PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

JANUARY 1878.

## XIX. *On a Form of Daniell Cell convenient as a Standard of Electromotive Force.* *By* OLIVER J. LODGE, *D.Sc.**

[Plate IV.]

ALTHOUGH a volt is the formal unit of electromotive force, yet it happens in practice that differences of potential get stated as equal to so many Daniell cells more frequently than any thing else, showing that there is some decided convenience in this mode of statement, a convenience partly owing, no doubt, to the fact that a freshly set-up Daniell is a tolerably uniform and easily reproduced standard. An ordinary Daniell, however, is by no means suitable as a standard, because of the diffusion of the copper-liquid through the porous cell. This defect must obtain in any cell where two liquids separated by a porous partition are employed; and hence attempts have been made to construct standard cells with solid electrolytes, or with mercury instead of copper salts, as in the little cell devised by Mr. Latimer Clark, which, though not absolutely constant, is still, I suppose, the best for its special purpose. But all cells with solid electrolytes are extremely inconstant, in the sense that they suffer greatly from short-circuiting and take some time to recover themselves; and there are some other inconveniences attending the use of a large number of Clark's cells.

* One of the cells was exhibited at a meeting of the Society in February 1877.

*A convenient Daniell Cell with high internal Resistance.*

Of all known cells, a Daniell charged with sulphate of zinc and sulphate of copper seems to be the most perfect—in this respect, that the materials remain always the same during action except that the sulphate of zinc gradually increases in quantity, a difference which scarcely affects the electromotive force. Almost the only defect in the constancy of a cell so charged is due to the fact that the two liquids diffuse into each other, for which reason the battery cannot retain its original state after it has stood for some time. Any thing equivalent to a porous partition is quite useless for keeping the liquids separate; and the only plan seems to be to provide as long a column of liquid as possible for the copper salt to diffuse through.

This is done in a compact and simple manner in the cell represented in fig. 1 (Pl. IV.). A wide-mouthed bottle (or a tall jar) is fitted with a cork through which passes a wide glass tube open at both ends. To the lower end of this tube a short closed tube (like a test-tube) is tied with silk thread ; a long strip of sheet zinc is put down the open tube; and a copper wire, recurved at the bottom and coated with sealing-wax except at its two ends, is passed through the cork to the bottom of the closed tube, where it is imbedded in a few crystals of copper sulphate. The bottle is then nearly filled with dilute sulphate of zinc, and the cork with the tube is inserted, the latter being so arranged that the end of the zinc strip and the mouth of the short tube are both below the surface of the liquid. In a short time a strong solution of sulphate of copper forms at the bottom of the closed tube where the copper wire is bare, and it gradually diffuses upward; but in order to reach the zinc it has to diffuse itself all through the water of the bottle and then up the long tube containing the zinc; and this takes a long time, though it certainly does take place to some extent in a week or so.

But when I want to put the cell by for any length of time, I pull the long tube a little higher up through the cork, so that the mouth of the short tube emerges above the liquid and thus entirely prevents diffusion. The zinc strip is also raised out of the liquid by the same action. It is convenient to have

the cork fitting pretty air-tight; or else evaporation may go on from the edges of the tube, and the salts which crystallize there may continue the diffusion slowly.

The copper wire need not be covered with wax or any thing; but if it were not, its upper parts would assist in the action until they were polarized; and hence the internal resistance would be liable to vary, which is not desirable. The internal resistance of such a cell is always rather high: for instance, in the one of which fig. 1 is a portrait, the bottle stands about 6 inches high, and the internal resistance is about 500 ohms when arranged as shown; but of course it depends greatly on the position of the tubes, and also somewhat on the temperature. Hence it is not to be regarded as giving necessarily a very constant current, but rather as a cell which can be used for a long time and yet keep its electromotive force nearly unchanged.

I have also made a set of small cells on the same principle, with ordinary quilled tubing for the tubes, and with test-tubes for the containing vessels, making the connexions by twisting the thin copper wire of one cell round the projecting tube (with the zinc bent down springily over it) of the next. A large number of such cells may be quickly made and arranged in ordinary test-tube stands; and they are convenient for many purposes, such as capacity- or insulation-testing, where high electromotive force is required*. The whole rack of cells was once accidentally upset; but though a little liquid escaped from the open ends of the zinc-tubes, the copper-liquid remained steady at the bottom of its tube without visible disturbance.

*A Cell for a Standard of Electromotive Force.*

Fig. 2 shows a bottle about 3 inches high, which I have made to act as a standard of electromotive force. It differs in no essential respect from fig. 1, except that the mouth of the tube containing the copper-solution never dips below the surface of the liquid, but always projects $\frac{1}{4}$ inch above it. The

* I suppose that by using platinum instead of copper wire, and strong nitric acid or else sulphuric acid and bichromate of potash instead of the copper salt, one could nearly double the electromotive force, though with some loss of constancy.

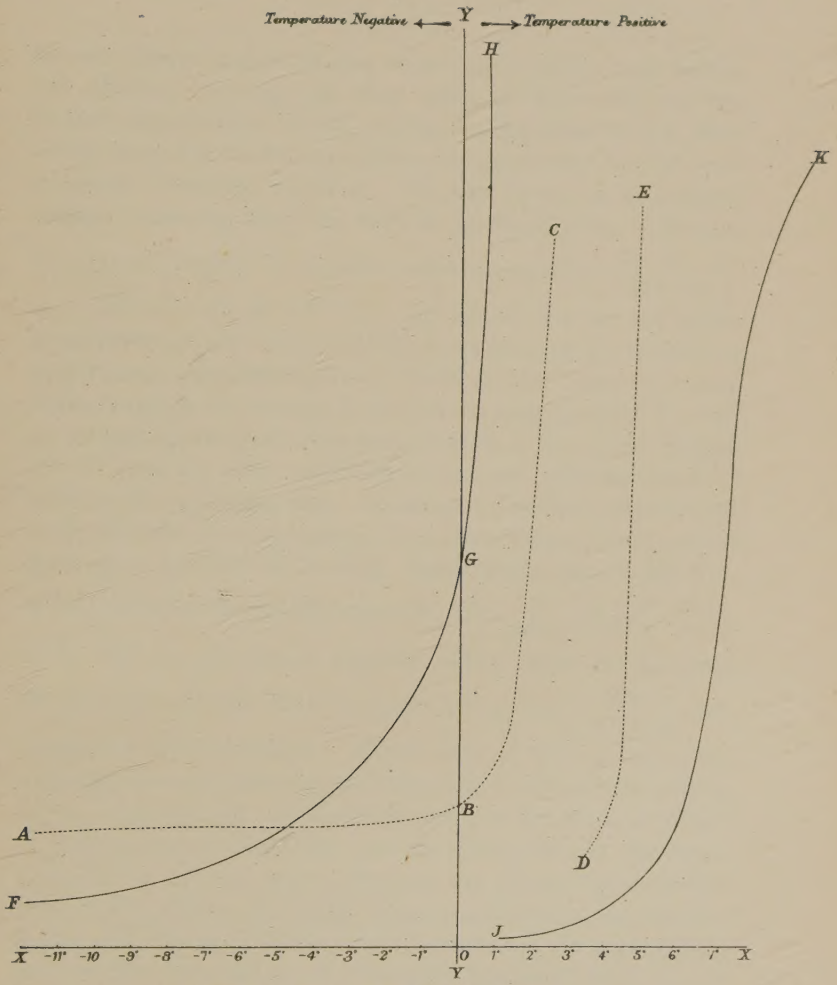
other or open tube does not project at all above the cork; and its lower end is drawn out and coiled round so as still further to retard the passage of the copper-liquid to the zinc. The zinc, which should be pure, is supported at the right height by a pin thrust through it. The closed tube is proportionally longer than in fig. 1; it is nearly filled with pure sulphate-of-copper solution, a few crystals being placed at the bottom; and it is tied to the other tube, as before, with silk thread (which appears not to rot). The copper wire is gutta-percha-covered with its ends bared. The bottle is filled nearly to its neck with very dilute sulphate of zinc; and the cork is then inserted air-tight.

No mixing of the liquids is now possible; but conduction still takes place over the damp surface of the glass tube, especially if, before use, the whole bottle be slightly inclined so as to wet the edges of the tube. The slight film of zinc-salt thus formed, being hygroscopic and being in a saturated atmosphere, will keep the top of the tube sufficiently moist for an immense time.

The only possible changes which can go on in this cell are in the zinc and the solution in immediate contact with it. This solution can at any time be drawn off with a pipette and replaced by fresh, without greatly affecting the liquid in the bottle (if the cork be air-tight); and the zinc can still more easily be taken out and replaced by a new piece.

I have described the cell as at present made; but if there were any chance of its coming into use as a standard, a few modifications might be introduced. Thus the zinc might be a short rod with an india-rubber collar fitting the tube and with a short copper wire attached to it, which should project above the cork instead of the zinc, the joint being a little way down the tube and protected by a coat of varnish from damp air. A set of experiments would have to be made to determine the dependence of electromotive force on temperature; and then a thermometer with a short scale might be fixed in each cork.

University College, London.




*Dotted Lines* ..... *are Capacity Curves.*
*Continuous Lines* ——— *are Conductivity Curves.*

*Point A about 0·002 micro-farads capacity per cubic centimetre, temperature −12°·0 C.*
*Point E about 0·1185 micro-farads capacity per cubic centimetre, temperature +5° C.*
*Point F about 2240 megohms resistance per cubic centimetre, temperature −12°·4 C.*
*Point K about 0·34 megohms resistance per cubic centimetre, temperature +11°·02 C.*

Ice as an Electrolyte.

W. E. Ayrton
John Perry

XX. *Ice as an Electrolyte.—Second Communication.* *By* W. E. Ayrton *and* John Perry, *Professors in the Imperial College of Engineering, Tokio, Japan.*

[Plate V.]

In our former paper on Ice as an Electrolyte, read before the Physical Society on May 26th of this year, we described experiments which proved, among other things, that as the temperature of ice is allowed to gradually rise the conductivity increases regularly, and that there is no sudden change in passing from the solid to the liquid state. We also determined roughly the specific inductive capacity of $\left\{\begin{matrix}\text{ice}\\\text{water}\end{matrix}\right\}$ at $-13°{\cdot}5$ C. and at $+8°{\cdot}7$ C., and found that at the latter temperature it was about 2240 times as great as in the former. Preliminary experiments also showed us that there was very little change in the specific inductive capacity up to 0° C.; and it was anticipated that there would not be a very great change after 0° ; we therefore concluded that a very great change must occur at the melting-point. A series of further experiments made with the same apparatus, since the writing of the previous aper, have enabled us to draw approximate curves (Pl. V.), A B C, D E for the specific capacity of $\left\{\begin{matrix}\text{ice}\\\text{water}\end{matrix}\right\}$ from $-12°{\cdot}2$ C. to $+5°$ C., all tests of capacity being made by charging for ten seconds and then short-circuiting the $\left\{\begin{matrix}\text{ice}\\\text{water}\end{matrix}\right\}$ condenser for fifteen seconds. From these it will be seen that, although the change at the melting-point is not quite as sudden as we expected, our anticipations are on the whole realized. It must be remembered too (see the description of the apparatus in our former paper) that, as the present experiments were made with a gradually rising temperature, the thermometer will always indicate a temperature a little higher than that of the ice ; the curve B C ought probably, therefore, to be even more vertical than it is.

Distances measured perpendicularly to Y O Y represent temperature—positive temperature if measured to the right, and negative if measured to the left. Distances measured perpendicularly to X O X represent specific capacity per cubic centi-

metre for points on the dotted lines, and conductivity for points on the continuous lines. The scale for temperature is the same for all the curves. The scale for vertical distances for the capacity-curve D E is one eighth of that for the capacity-curve A B C; and the scale for vertical distances for the conductivity-curve J K is three thousandths of that for the curve F G H.

The point A corresponds with a capacity per cubic centimetre of about 0·002 microfarad, at −12 C; E corresponds with a capacity per cubic centimetre of about 0·1185 microfarad, at +5° C.; at this apparent temperature the capacity was increasing so rapidly as to make exact measurements very difficult, although the temperature was increasing but slowly. The point F corresponds with a specific resistance per cubic centimetre of about 2240 megohms, at −12°·4 C.; and K with a resistance per cubic centimetre of about 0·34 megohm, at +11°·02 C. As in our previous experiments, the water employed in the water-condenser was distilled, and the ice was formed by freezing it with an external freezing-mixture no particle of which was allowed to fall into the distilled water.

The important theory which Prof. Clerk Maxwell has developed, by comparing the propagation of electro-magnetic disturbances through the ether with the propagation of light-vibrations, has been illustrated only by paraffin (a non-conductor); and he has not considered the propagation of electro-magnetic disturbances in a conducting medium. But according to a former paper of ours, on the "Viscosity of Dielectrics," no dielectric can be assumed to be non-conducting, and the charging of any condenser whatever is always accompanied with absorption phenomena; also absorption certainly increases with conductivity.

Hence although, if a method of experimenting were employed in which a water-condenser of great internal resistance were discharged through wires of less and less resistance for shorter and shorter periods of time, the measured specific inductive capacity might get less and less, and gradually approach a value equal to the square of the index of refraction of water for infinitely long luminous waves (the index of refraction for air being called unity), still practically the measured specific

inductive capacity can never be even approximately equal to the refractive index of water, since the absorbed charge is immeasurably greater than the surface-charge. We therefore need not expect to find the specific inductive capacity of water in its variations with temperature consistent with Dr. Gladstone's results for index of refraction. When Prof. Clerk Maxwell takes into account conductivity, his equations are not generally integrable; but even if they were they could not deal with the real case, because he leaves absorption quite out of account.

July 30, 1877.

---

XXI. *On a Method of measuring the Absolute Thermal Conductivity of Crystals and other rare Substances.*—Part I. *By* Oliver J. Lodge, *D.Sc.**

1. When only a small portion of a substance is obtainable on which to experiment, the measure of conductivity by any dia-calorimetric method becomes difficult; and accordingly observers have contented themselves, in the case of the rarer crystalline bodies, with comparing their conductivities in different directions by Sénarmont's or some similar method. If the substance is sufficiently plentiful to be obtained in slabs (like rocks), then some modification of Fourier's "thermomètre de contact" will give its conductivity, though there are many objections to the use of this instrument.

But there is another method of Fourier's, applicable only to long rods, put in practice by Biot, Despretz, Forbes, and recently by Wiedemann and Franz (commonly known as Forbes's method), which it seems possible to modify so as to make it applicable to short rods or even slices†. This well-known method consists

* Read January 19, 1878.

† The method occurred to me when thinking how best to measure the conductivity of tourmaline in opposite directions along the axis, a subject which I was considering in conjunction with Mr. S. P. Thompson of Bristol; for we had reason to think that tourmaline and all other pyroelectric crystals must necessarily possess a unilateral conductivity along their axis both for heat and electricity; and this supposition has been partially confirmed, in the case of heat, by some preliminary experiments of Mr. Thompson's last summer on a very small crystal. No further con-

in observing the permanent curve of temperature along a cylindrical rod of the given material, one end of which is heated and the rest exposed to the atmosphere. Let $s$ be the area of cross section (which need not be circular) and P the perimeter of the rod, the latter being defined as the length of a string wrapped once round the rod if the actual perimeter is any *re-entrant* curve. The condition to be expressed is that the total gain of heat of any element of the rod by its anterior, posterior, and exterior surfaces is equal to nothing. Taking $t$ as the excess of temperature over that of the air of an element in a position $x$ along the rod, we have the quantity of heat

$-ks\dfrac{dt}{dx}$ entering at its anterior or hotter surface in unit time,

$+ks\left(\dfrac{dt}{dx}+\dfrac{d^2t}{dx^2}dx\right)$ at its posterior surface,

$-\mathrm{P}ht\,dx$ at the surface exposed to the air;

$k$ being the conductivity of the rod, and $h$ the radiation-coefficient of its surface, *i. e.* the quantity of heat lost in unit time by unit surface when it is one degree hotter than the air.

Putting the sum of these quantities equal to zero, we have

$$\frac{d^2t}{dx^2}=\frac{\mathrm{P}h}{sk}t=p^2t \text{ say,}$$

an equation whose complete integral is

$$t=\mathrm{C}_1e^{px}+\mathrm{C}_2e^{-px},$$

or, as I shall prefer to write it,

$$t=\mathrm{A}\cosh px-\mathrm{B}\sinh px.$$

The constants A and B are determined in terms of $p$ as soon as one knows the temperatures of any two points of the rod; and the temperature of a third point will determine $p$, whence, if $h$ be separately found, $k$ is known. Thus, suppose we know $t_0$ the temperature of the origin, and $t_x$ the temperature of a

---

firmation or modification of the experiment, however, has yet been possible, owing to the scarcity of the crystal and the difficulty of obtaining a large slice; but this difficulty has now been removed by the kindness of Professor Nevil Story Maskelyne.

point at a distance $x$ from it, and also $t_{\frac{1}{2}}$ the temperature halfway between these points; then

$$A = t_0,$$
$$B = t_0 \coth px - t_x \operatorname{cosech} px,$$
$$p = \frac{1}{x} \cosh^{-1} \frac{t_0 + t_x}{2t_{\frac{1}{2}}}.$$

2. Now suppose the rod to be cut in half and a slice of crystal or any substance interposed: the curve of temperature will have a discontinuity at the junctions; but if the curve along each rod is observed, it may be possible to calculate it for the crystal. The method which I propose, then, is to cut a cylindrical piece of the substance to be examined, of length $z$, with flat faces, and to squeeze it between two copper or iron rods (or any other metal whose conductivity is well known) of exactly the same cross section as itself both in shape and size, putting a pad of a few thicknesses of tinfoil between the surfaces, so as to make better contact, and then to observe the curve of temperature down each rod when one end of one is heated and the further end of the other is cooled, the whole having been left long enough to attain a permanent state.

*Conduction through a cylinder inserted between a pair of metal rods.*

3. Let the cylinder be of length $z$,

and conductivity $\chi$,

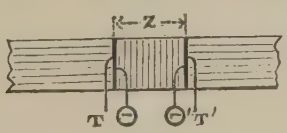
and let its surface have the radiation-coefficient $h'$.

Let the packing on each side be of thickness $y$,

and conductivity $\kappa$,

and let it be so thin that radiation from its edge is negligible.

Fig. 1.




Also let us take the atmospheric temperature as an artificial zero; so that by "temperature" we shall always mean *excess of temperature* above that of the air.

And let T and $\Theta$ be the temperatures of metal and cylinder on each side of first packing (see fig. 1),

T′ and $\Theta'$ ditto on each side of second packing.

Then the quantity of heat which leaves the first rod traverses the first packing and enters the cylinder, which is expressed analytically thus ($k$ being the conductivity of the metal),

$$k\frac{d\mathrm{T}}{dx}=\kappa\frac{\Theta-\mathrm{T}}{y}=\chi\frac{d\Theta}{dx}; \quad \ldots \quad (1)$$

and similarly for the quantity which crosses the second packing,

$$k\frac{d\mathrm{T}'}{dx}=\kappa\frac{\mathrm{T}'-\Theta'}{y}=\chi\frac{d\Theta'}{dx}, \quad \ldots \quad (2)$$

four equations from which the unknown quantities, $\frac{\kappa}{y}$, $\Theta$, and $\Theta'$ can be eliminated, and $\chi$ be found.

Now the curve of temperature down the cylinder is, from § 1,

$$\theta=\Theta\cosh qx-(\Theta\coth qz-\Theta'\operatorname{cosech} qz)\sinh qx, \quad (3)$$

where

$$q^2=\frac{\mathrm{P}h'}{s\chi}, \quad \ldots \quad (4)$$

hence

$$\frac{d\Theta}{dx}=q(\Theta'\operatorname{cosech} qz-\Theta\coth qz)$$

and

$$\frac{d\Theta'}{dx}=q(\Theta'\coth qz-\Theta\operatorname{cosech} bz).$$

Substituting these values in equations (1) and (2) and eliminating, we get from the first and second of each set

$$\Theta\frac{d\mathrm{T}'}{dx}+\Theta'\frac{d\mathrm{T}}{dx}=\mathrm{T}\frac{d\mathrm{T}'}{dx}+\mathrm{T}'\frac{d\mathrm{T}}{dx}=\frac{d}{dx}(\mathrm{TT}');$$

also from the first and third of each set,

$$\Theta=\frac{k}{\chi q}\left(\frac{d\mathrm{T}'}{dx}\operatorname{cosech} qz-\frac{d\mathrm{T}}{dx}\coth qz\right),$$

$$\Theta'=\frac{k}{\chi q}\left(\frac{d\mathrm{T}'}{dx}\coth qz-\frac{d\mathrm{T}}{dx}\operatorname{cosech} qz\right).$$

Therefore, combining all these,

$$\frac{\chi q}{k}\sinh qz=\frac{\left(\frac{d\mathrm{T}'}{dx}\right)^2-\left(\frac{d\mathrm{T}}{dx}\right)^2}{\frac{d}{dx}(\mathrm{TT}')}. \quad \ldots \quad (5)$$

Hence $\chi$ is determined in terms of $q$, which itself contains it together with the radiation-coefficient $h'$ (which must be supposed known). We may write the last equation thus, by (4),

$$\frac{\sinh qz}{q} = \frac{sk}{Ph'} \cdot \frac{\left(\frac{dT'}{dx}\right)^2 - \left(\frac{dT}{dx}\right)^2}{T\frac{dT'}{dx} + T'\frac{dT}{dx}}, \quad \ldots \quad (6)$$

which shows that this method is not satisfactory for determining $q$ when the product $qz$ is very small.

4. Now there are three special cases, depending on the value of $h'$:—

(1) When the crystal has its natural surface, and the value of $h'$ is determined by special experiment on its rate of cooling. In this case the above equation remains as written, and may be treated, as $qz$ will generally be less than unity, by expanding the left-hand member,

$$z\left(1 + \frac{q^2z^2}{\underline{3}} + \frac{q^4z^4}{\underline{5}} + \ldots\right),$$

and then solving for $q$ by successive approximations.

(2) When the crystal and rods are covered over with a coat of varnish (as Brunswick black), so that $h$ is the same for all. In this case the coefficient of the right-hand member becomes simply $\frac{1}{p^2}$, but the treatment required is the same as in the first case.

(3) When the crystal is surrounded with cotton-wool or felt, or in some other way has its exterior surface made adiabatic, so that $h' = 0$. In this case the left-hand member of the above equation equals $z$, and the right-hand becomes indeterminate, so that a fresh investigation is necessary.

*Case when radiation from the exterior surface of the cylinder is prevented.*

5. Heat will now flow through the cylinder as through part of an infinite wall, and $\frac{d\theta}{dx}$ becomes simply $\frac{\Theta' - \Theta}{z}$; hence the two sets of equations (1) and (2) are now all equal to one

another, and they reduce to

$$k\frac{dT}{dx}=\epsilon\frac{\Theta-T}{y}=\chi\frac{\Theta'-\Theta}{z}=\epsilon\frac{T-\Theta'}{y}. \quad . \quad . \quad (7)$$

But as these are only three equations between four unknown quantities, some further observation is necessary before we can determine $\chi$. We may either omit the crystal altogether, or, what is probably better, replace it by a piece of the same metal as the rods are made of, and repeat the temperature-determinations, the packing being kept exactly the same as before. Denoting the temperatures in this case by small letters, the equations will now be

$$k\frac{dt}{dx}=\epsilon\left(\frac{\theta-t}{y}\right)=k\frac{\theta'-\theta}{z}=\epsilon\left(\frac{t-\theta'}{y}\right). \quad . \quad . \quad (8)$$

where $z$ is the thickness of the bit of metal. By these three equations $\frac{\epsilon}{y}$ is determined; and its value may then be substituted in the former set.

*Eliminating* $\Theta$ *and* $\Theta'$ from the former set (7), we have

$$\left(\frac{zk}{\chi}+\frac{2yk}{\epsilon}\right)\frac{d^2T}{dx^2}=T-\mathrm{T}; \quad . \quad . \quad . \quad . \quad (9)$$

similarly from (8) we get

$$\left(\frac{zk}{\chi}+\frac{2yk}{\epsilon}\right)\frac{d^2t}{dx^2}=t-\tau,$$

whence, getting rid of $\frac{\epsilon}{y}$, we have

$$z\frac{k}{\chi}=z+\frac{\mathrm{T}-\mathrm{T}}{\frac{d^2\mathrm{T}}{dx^2}}-\frac{t-\tau}{\frac{d^2t}{dx^2}}; \quad . \quad . \quad . \quad (10)$$

so $\chi$ is determined by the two observations.

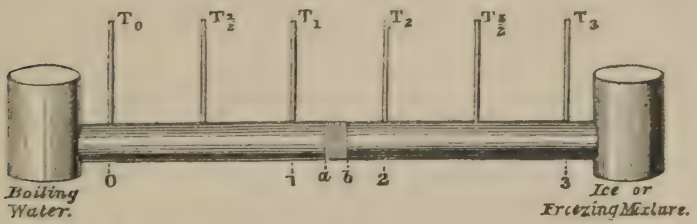
6. Although this method requires two experiments while the other [§ 5] required only one, yet it will probably be more useful than the other, as it is applicable to very thin slices of crystals—in fact the thinner the better,—whereas the other one applies more to substances in the form of a short rod; for it fails when $z$ becomes very small. This failure of the § 5 method is due to the fact that it depends entirely on radiation going on from the cylinder at the same intrinsic rate as from the rods,

and on some appreciable quantity of heat being lost in this way during its passage through the cylinder; hence of course a certain *length* of the cylinder is essential.

*Observation of the Curves of Temperature.*

7. We have seen (§ 1) that, to determine the curves of temperature, it is necessary to know the actual temperature of two, or perhaps one should rather say three, points on each rod. Each rod then should have three holes bored to receive the bulb of a thermometer, one near each end, at a distance $l$ from one another, and another in the middle halfway between the other two. Let the temperatures which these thermometers indicate above that of the air at the time be denoted by $T_0$, $T_{\frac{1}{2}}$, $T_1$, $T_2$, $T_{\frac{5}{2}}$, $T_3$, their position being shown in the figure.

Fig. 2.




Denote the distances thus:—

$$0\,a = l_0 = b\,3,$$
$$1\,a = l_1 = b\,2,$$
$$0\,1 = l = 2\,3 = l_0 - l_1;$$

then measuring $x$ from 0 for the first rod, we have as the curve of temperature down it, by § 1,

$$t = \mathrm{A}\cosh px - \mathrm{B}\sinh px;$$

that is,

$$t = \mathrm{T}_0\cosh px - (\mathrm{T}_0\coth pl - \mathrm{T}_1\,\mathrm{cosech}\,pl)\sinh px \quad . \quad (11)$$

similarly down the second rod, reckoning $x$ from $b$, the curve is

$$t' = \mathrm{A}'\cosh px - \mathrm{B}'\sinh px,$$

where

$$\mathrm{A}' = \frac{\mathrm{T}_2\sinh pl_0 - \mathrm{T}_3\sinh pl_1}{\sinh pl},$$

and

$$\mathrm{B}' = \frac{\mathrm{T}_2\cosh pl_0 - \mathrm{T}_3\cosh pl_1}{\sinh pl}. \quad . \quad . \quad . \quad (12)$$

We can now at once express the values of the "known quantities" which occur in equations (1) and (2), and in the right-hand members of equations (5) and (6), viz.

$$\mathrm{T},\quad \mathrm{T}',\quad \frac{d\mathrm{T}}{dx},\text{ and }\frac{d\mathrm{T}'}{dx}.$$

Thus

$$\left.\begin{aligned} \mathrm{T} &= \frac{\mathrm{T}_1 \sinh pl_0 - \mathrm{T}_0 \sinh pl_1}{\sinh pl}, \\ \frac{d\mathrm{T}}{dx} = \left(\frac{dt}{dx}\right)_{x=l} &= p\,\frac{\mathrm{T}_1 \cosh pl_0 - \mathrm{T}_0 \cosh pl_1}{\sinh pl}, \\ \mathrm{T}' = \mathrm{A}' &= \frac{\mathrm{T}_2 \sinh pl_0 - \mathrm{T}_3 \sinh pl_1}{\sinh pl}, \\ \frac{d\mathrm{T}'}{dx} = \left(\frac{dt'}{dx}\right)_{x=0} &= -p\mathrm{B}' = p\,\frac{\mathrm{T}_3 \cosh pl_1 - \mathrm{T}_2 \cosh pl_0}{\sinh pl}. \end{aligned}\right\} \quad (13)$$

The symmetry of these expressions is visible in the following, where for shortness $\mathrm{sh}_0$ is written instead of $\sinh pl_0$, $\mathrm{ch}_1$ for $\cosh pl_1$, and so on :—

$$\mathrm{T} : \frac{d\mathrm{T}}{pdx} : 1 = \begin{vmatrix} \mathrm{sh}_0 & \mathrm{sh}_1 \\ \mathrm{T}_0 & \mathrm{T}_1 \end{vmatrix} : \begin{vmatrix} \mathrm{ch}_0 & \mathrm{ch}_1 \\ \mathrm{T}_0 & \mathrm{T}_1 \end{vmatrix} : \begin{vmatrix} \mathrm{sh}_0 & \mathrm{sh}_1 \\ \mathrm{ch}_0 & \mathrm{ch}_1 \end{vmatrix} \quad (14)$$

8. We can now write down the value of the right-hand member of equation (5) thus,

$$\frac{\left(\frac{d\mathrm{T}'}{dx}\right)^2 - \left(\frac{d\mathrm{T}}{dx}\right)^2}{\mathrm{T}\frac{d\mathrm{T}'}{dx} + \mathrm{T}'\frac{d\mathrm{T}}{dx}} = \frac{p^2\{(\mathrm{T}_3 \mathrm{ch}_1 - \mathrm{T}_2 \mathrm{ch}_0)^2 - (\mathrm{T}_1 \mathrm{ch}_0 - \mathrm{T}_0 \mathrm{ch}_1)^2\}}{p(\mathrm{T}_1\mathrm{T}_3 - \mathrm{T}_0\mathrm{T}_2)\sinh pl}; \quad (15)$$

so (6) becomes

$$\frac{\sinh qz}{q} = \frac{h}{h'} \cdot \frac{\{(\mathrm{T}_0 + \mathrm{T}_3)\cosh pl_1 - (\mathrm{T}_1 + \mathrm{T}_2)\cosh pl_0\}\{(\mathrm{T}_0 - \mathrm{T}_3)\cosh pl_1 - (\mathrm{T}_1 - \mathrm{T}_2)\cosh pl_0\}}{(\mathrm{T}_0\mathrm{T}_2 - \mathrm{T}_1\mathrm{T}_3)\sinh pl}, \quad (16)$$

which is a form convenient for calculation.

9. So also for the second method (§ 5), we can write down the value of the quantities occurring in the right hand of (10),

$$\frac{\mathrm{T}' - \mathrm{T}}{\frac{d\mathrm{T}}{dx}} = \frac{(\mathrm{T}_1 - \mathrm{T}_2)\sinh pl_0 - (\mathrm{T}_0 - \mathrm{T}_3)\sinh pl_1}{p(\mathrm{T}_0 \cosh pl_1 - \mathrm{T}_1 \cosh pl_0)}; \quad (17)$$

and similarly for the small $t$'s, of which a set $t_0$, $t_1$, $t_2$, $t_3$ have been observed. In this case there is no loss of heat in passing through the crystal; so we ought to have

$$\frac{dT}{dx} = \frac{dT'}{dx},$$

which gives the condition

$$\frac{T_1 + T_2}{T_0 + T_3} = \frac{\cosh pl_1}{\cosh pl_0}; \quad \ldots \quad (18)$$

and unless this condition is satisfied there is some error in the experiment, and it is useless to proceed.

I have to express my thanks to my brother, Mr. Alfred Lodge, of St. John's College, Oxford, for several suggestions in the writing out of the above and for some improvements in the notation.

In the second part of this communication some practical details will be given, together with the results of some trials of the method now going to be made.

University College, London.

---

## XXII. *On Permanent Plateau's Films.*

*By* SILVANUS P. THOMPSON, *B.Sc. B.A.**

1. THE film-figures, which occupy so large a part of the researches of Plateau† upon the Molecular Statics of Liquids, when prepared with the glyceric fluid prescribed by their discoverer, are of extreme fragility and of short duration. With such a liquid films have been made which lasted ten, twelve, or even sixteen hours in the air, and from fifteen to thirty hours when protected by an external vessel of glass. In one instance‡, where chloride of calcium had been added to the liquid, a duration exceeding fifty-four hours was observed. The average duration of the films, especially if they are to be exhibited to a number of persons, is more brief.

No method hitherto described of producing these films in a

* Read December 15, 1877.

† *Statique expérimentale et théorique des liquides soumis aux seules forces moléculaires.* Par J. Plateau. Gand et Leipzic: 1873

‡ Plateau, *op. cit.* vol. i. p. 175, § 106.

more durable or permanent form has been quite satisfactory, though there have been several attempts. Of these the writer was not aware when he began the present investigation, though most of them are mentioned in the later chapters of Plateau's work already named. A brief enumeration of these attempts will therefore preface a description of the process now announced for rendering the films permanent.

2. M. Plateau has himself endeavoured* to fix the film-figures by dipping the wire frames into solutions which evaporate, leaving films of greater or less tenacity. He was unsuccessful with collodion and with albumen. A solution of gutta-percha in bisulphide of carbon gave better results. The system of films upon a cubical frame of 2 centims. side was preserved for several months, but eventually fell to powder. The same substance refused to form a film upon a frame of 3 centims. side. Glass, which in the single instance of the spherical film or bulb is so familiar, presents too many difficulties to be applicable for the production of the film-figures.

Schwartz† succeeded with much ingenuity in obtaining the anticlastic film-surface upon a skew quadrilateral frame whose sides were 3·5 centims. long, with gelatine.

Prof. Mach‡ imitated the system of films developed upon a tetrahedral frame with thin laminæ of caoutchouc covering the sides, and drawn together when the air was exhausted from within.

Better results have been yielded by viscous liquids which solidify at temperatures moderately low.

M. Rottier, of Ghent, has obtained films of considerable dimensions with a mixture, suggested by Böttger in 1838 for blowing bubbles, consisting of 8 parts of resin (colophony) with 1 of linseed oil, and fusing at 97°. But the films were always found after a few hours to have broken by contraction.

Mach§ has obtained films upon a tetrahedral frame of 5 centims. side dipped in fused resin. He has also obtained films

* Plateau, *op. cit.* vol. ii. p. 119, § 311.

† Ibid. vol. i. p. 233, § 141.

‡ *Die Gestalten der Flussigkeiten.* Prag.: 1872. See also Plateau, *op. cit.* vol. ii. p. 374, § 210 *bis.*

§ *Wiener akademischer Anzeiger,* 1862, vol. xlvi. 2nd part, p. 125, "Ueber die Molecularwirkung der Flussigkeiten."

from solutions of alkaline silicates which hardened on exposure to the air.

M. Plateau* has found a mixture of 5 parts of resin with 1 part of gutta-percha superior to resin alone. A system of films upon a cubical frame of 5 centims. side, prepared by M. Donny, was preserved for two years, but ultimately fell into fragments.

3. The author's first experiments were made with pure amber-coloured resin fused. The resulting films were brittle and of irregular thickness. When 10 per cent. of turpentine was added, the liquid was too mobile at high temperatures to form films, and at low temperatures too stiff to form them regularly.

A mixture of pure resin with Canada balsam was tried, with good results; and a series of experiments followed, to ascertain the best proportions. When the mixture contained a less proportion of balsam than 35 per cent. the films were too brittle, and irregular in form. If it contained more than 70 per cent. of balsam the films did not readily harden, and were not formed without difficulty. A mixture of 55 per cent. of resin with 45 of balsam, which fused about 85°, gave good films, tough on cooling, but somewhat brittle. The mixture yielding the most satisfactory results contained 46 per cent. of resin and 54 of balsam. This mixture is sufficiently fused at 80° to be workable, but yields the best films at 93° to 95°. At 105° films can be obtained; and they are thinner than those formed from the more viscid fluid at 95°. At 110° films are still obtainable; and they frequently exhibit chromatic phenomena, but usually burst before hardening.

[The specimens exhibited to the Society are made with this mixture. They include a cubical frame of 2·5 centims. side, and a tetrahedral frame of 3·1 centims. side. Larger specimens have been obtained, however, though they generally show some imperfection of form. I have had a flat circular frame of 11 centims. diameter covered with a film of beautiful transparency. Brass wire appears better than iron for the frames.]

The films made with the mixture described are remarkably tough, and if preserved from rough handling appear to be of

* *Op. cit.* vol. ii. p. 119, § 314.

indefinite durability. A number of frames holding films have been hanging for over two months unprotected upon the wall of the laboratory of the writer, and are still intact. Brass wire of 0·33 millim. in diameter has been employed for the construction of the frames. When a thicker wire is used, the films become irregular from the longer retention of heat by the wire, and the consequent earlier cooling of the central portions of the films.

As with the soap-films, so with those of resinous matter, success depends largely upon the purity of material employed. Dust and oily matters must be scrupulously excluded; and the resin should be retained at a temperature near its boiling-point for some time, to purify it of more volatile matter, before the balsam is mixed with it.

The most perfect films are obtained when the wire frames, after being dipped in the liquid, are removed to an air-bath at the temperature of about 80°, in which they are left, and the whole is allowed slowly to cool.

In proof of the toughness of the films, it may be mentioned that a recent flat film upon a circular frame of 4 centim. diameter of iron wire of 0·9 millim. gauge sustained, without breaking, the pressure of a cylindrical fifty-gramme weight, of 24 millims. diameter, placed upon its centre.

---

## XXIII. *On* Grove's *Gas-Battery.*

*By* Henry Foster Morley, *M.A.*, *B.Sc.**

It appears to me that the question as to the mode of action of the well-known gas-battery has not yet been definitely settled.

1. The discoverer says, "The chemical or catalytic action can only be supposed to take place, with ordinary platina-foil, at the line or water-mark where the liquid, gas, and platina meet"†. Nevertheless he showed that water containing oxygen in one tube and hydrogen gas in the other tube gave a conti-

* Read March 16, 1878.

† Phil. Mag. December 1842. See also Phil. Trans. 1843, p. 107.

nuous current*. As regards exp. 29 in the last-quoted excellent paper (viz. the experiment in which, hydrogen being in one tube and nitrogen in the other and no oxygen being dissolved in the liquid, hydrogen was found to appear in the nitrogen tube), as Mr. Grove does not say that there is a current, and as the presence of a current would contradict the conservation of energy, I am inclined to think that the effect is due to diffusion, and that it would occur whether the platinums were joined or not.

2. Mr. Justice Grove says that the phenomenon does not take place when the nitrogen is absent and its place filled by the liquid; and this is just what we should expect if the effect is due to diffusion. Mr. Grove thought it just possible that the hydrogen decomposed the water in its tube, combining with the oxygen, and that an equal amount of hydrogen was liberated in the other tube. Since the total amount of water is not changed, it is clear that such a decomposition could not be accompanied by a current.

3. Nevertheless Dr. Schönbein said that pure water containing no oxygen in one tube and an aqueous solution of hydrogen in the other gave a continuous current†. M. Gaugain makes the same assertion, but adds that he deprived his water of air by boiling‡. To boil water and then let it stand in the air is evidently not enough to deprive it of oxygen; hence these anomalous results may be due to the water not having been absolutely free from oxygen. Such a current, as before stated, would contradict conservation of energy: indeed it has been shown by Mr. Grove that water absolutely free from oxygen in one tube and hydrogen gas in the other tube produces no current§.

4. In one experiment Mr. Grove arranged his platinum plates, which I believe were platinized, in such a way as just to cut the surface of the liquid in the tubes: he got a strong current until the liquid rose above the platinum, when it became very weak. M. Gaugain says, and, I think, rightly, that this is due to the greater thickness of liquid through which the

* Phil. Trans. 1843, exp. 28 &c.

† Phil. Mag. March 1843.

‡ *Comptes Rendus*, February 25, 1867; Phil. Mag. June 1867.

§ Phil. Trans. 1843, exp. 7 and elsewhere.

gas must now pass in order to get at the platinum—when the platinum is partly exposed the film along the line of junction being extremely thin.

5. M. Gaugain made a cell in which the platinum plates were movable, and determined, by the method of opposition, the electromotive force when the plates were partly exposed; he then lowered them until they were wholly immersed, and determined the electromotive force immediately. In this experiment the current was only allowed to flow for a few seconds. He found that the two determinations were the same, and concluded that the action of the battery depends entirely upon dissolved gas. It is, however, open to any one to assert that the platinums, when lowered, retained minute bubbles of gas on their surface, and that thus there were still many points of contact of liquid, gas, and platinum.

6. M. Gaugain, following Dr. Schönbein, asserts that "the oxygen serves simply to depolarize the positive wire," and "that its function is that of sulphate of copper in Daniell's cell"—in other words, that, were it not for the opposition current developed by the freshly-deposited hydrogen, the current could be kept up indefinitely without the presence of oxygen. As I have before stated, I cannot conceive this state of things.

---

I. In order to show that some, at all events, of the current in the gas-battery is due to dissolved gases, I made the following experiments in the laboratory of Professor Carey Foster:—A gas-couple with wholly submerged non-platinized platinum plates was charged by electrolysis and short-circuited for a week, after which the lengths of the columns of oxygen and hydrogen were read off by means of a telescope on different days, the couple being all the while short-circuited. A similar couple, from which the platinum plates were removed after it had been charged, was similarly treated.

The barometer-reading was, of course, corrected for expansion, for the column of liquid below the gas, and for aqueous tension, the slight effect of sulphuric acid on the aqueous tension being neglected. A correction was applied for the curved ends of the tubes, and the corrected lengths reduced to 0° C. 760 millims.

The result in millimetres for the couple without platinum plates was:—

| | Nov. 13. | Dec. 11. | Jan. 9. |
|---|---|---|---|
| Hydrogen ..... | 76·3 | 76·4 | 76·3 |
| Oxygen ..... | 49·9 | 49·7 | 49·7 |

Practically the volume of the gas in these tubes was not altered by diffusion.

For the tubes which contained the platinum plates the lengths were:—

| | Nov. 13. | Nov. 20. | Dec. 11. | Jan 9. |
|---|---|---|---|---|
| Hydrogen ..... | 56·5 | 56·6 | 55·5 | 55·0 |
| „ calculated | 56·6 | 56·4 | 55·75 | 54·9 |
| Oxygen ..... | ... | 34·4 | 34·2 | 34·2 |

The second line is calculated from the first by least squares, on the assumption of a uniform decrease of hydrogen. The greatest error is $\frac{1}{4}$ millim.; and 1·7 millim. has disappeared. The oxygen seems to have been supplied by the air.

II. On December 11 I joined the plates through a galvanometer of 6917 ohms resistance. The connexion through the galvanometer was made without previously breaking the circuit; yet a current was instantly shown; after 19 hours the deflection was $20\frac{1}{2}$ divisions. By comparison with a Daniell's cell whose electromotive force I assumed to be 1·1, I found that a deflection of 1 division indicated a current of ·00000000056 electromagnetic unit.

If we assume that the current in the short circuit is the same as that passing through the galvanometer, an assumption which later experiments show to be not far from the truth, we shall find that 8 cubic millims. of hydrogen per week would be required to keep up this current. Now 32 cubic millims. have actually disappeared per week. The difference may be partly due to the inaccuracy of the assumption just made, and partly to the fact that some of the hydrogen combines with oxygen that has found its way from the air into the hydrogen-tube, the local currents thus produced not contributing to the main current.

III. An experiment similar to I., in which, however, the gases were prepared chemically, and in which there was also a gas-couple whose plates were not joined by a wire. The lengths of gas, in millimetres, corrected as before, were:—

For the couple with joined plates—

| | May 8. | Oct. 4. | Loss. | Volume lost, in cubic centims. |
|---|---|---|---|---|
| Hydrogen ..... | 146·1 | 136·0 | 10·1 | 2·87 |
| Oxygen ..... | 64·8 | 62·7 | 2·1 | ·60 |

For the couple with unjoined plates—

| | May 8. | Oct. 4. | Loss. | Volume lost, in cubic centims. |
|---|---|---|---|---|
| Hydrogen ..... | 63·9 | 59·3 | 4·6 | ·90 |
| Oxygen ..... | 64·1 | 61·3 | 2·8 | ·44 |

For the couple with no plates—

| | | | | |
|---|---|---|---|---|
| Hydrogen ..... | 92·0 | 86·9 | 5·1 | 1·30 |
| Oxygen ..... | 90·0 | 85·4 | 4·6 | ·74 |

In this case a good deal of gas seems to have been lost by diffusion.

The ratio of hydrogen lost to oxygen lost in the three cases is 4·8, 2·1, and 1·8 respectively. If we assume that 1·8 is the ratio of the gases lost through diffusion, and that the loss of oxygen in the first two cases is due solely to this cause, we shall find that 1·79 and ·11 cubic centim. of hydrogen respectively still remain to be accounted for in the two cases. I attribute this loss to local currents in the second case, and partly to these but chiefly to the main current in the first case, most of the necessary oxygen being supplied by the air to the liquid.

IV. If the hydrogen in a gas-couple with submerged plates be warmed by the hand, the current is increased; and if it be cooled the current is diminished: indeed it is very sensitive to changes of temperature, and of pressure also; and hence it is hardly possible to determine its strength with much accuracy. The further any horizontal layer of liquid in the hydrogen-tube is from the gas, the less hydrogen does it contain. Any expansion of the gas from heat or decrease of pressure brings a more saturated solution into contact with the immersed plate and the current increases, whereas contraction produces the opposite effect.

V. When a cell has been recently charged by electrolysis the current is at first very strong; but it soon falls off, and at last remains of nearly constant strength. This is because the water was at first saturated with the gas, but this gas being

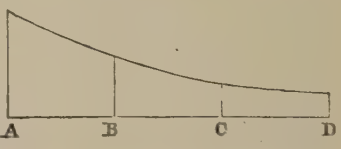
used up by the current takes some time to be restored by solution at the surface, and when equilibrium is attained the liquid round the plate will contain less dissolved gas the further it is from the surface. M. Gaugain attributed the falling-off in the strength of the current to the deposition of hydrogen on the positive plate; there is no need, however, for any such supposition. I employed a battery in which the plates were wholly immersed; and the final current varied with the depth of the top of the plate in the hydrogen-tube from the surface, and with the resistance in circuit, as the following Table shows:—

| $n$. | C. | R. | E. | C calc. |
|---|---|---|---|---|
| 63 | 25 | 240,000 | 6,000 | 25 |
| 63 | 39 | 11,600 | 450 | 40 |
| 63 | 42 | 1,600 | 67 | 41 |
| 8 | 203 | 10,200 | 2,070 | 204 |
| 7·5 | 217 | 200 | 43 | 219 |
| 10·5 | 201 | 10,200 | 2,050 | 196 |
| 11 | 81 | 244,500 | 19,800 | 81 |
| 0 | 277 | 10,200 | 2,800 | 224 |
| −0 | 91 | 234,500 | 21,300 | 89 |

$n$ is the distance between top of plate in hydrogen-tube and surface of liquid in that tube, in millimetres; $n=0$ means that the plate cuts the surface and rises about 1 millim. above it: the plate in the oxygen tube was always at the same distance from the surface and was wholly immersed. C is the final strength of current, usually several hours after introducing resistance, given in deflections of the galvanometer, each of which is about ·00000073 weber. R is the resistance in ohms; and E is $\frac{CR}{1000}$.

In the first place, it seems that the increase of current consequent on causing the platinum to cut the surface is too slight to oblige us to assume that a new force is thereby brought into action; in other words, the whole of the current in the gas-battery is due to dissolved gas.

I tried to express the relation between $n$, C, and E in a formula obtained theoretically. In this I had but little success; but perhaps I may venture briefly to indicate the results:—




Let abscissæ represent depth below surface; ordinates quantity of hydrogen in solution at any level, supposed uniform. Imagine a tube of uniform bore open to air at D and to hydrogen at A. Suppose B C a uniform platinum rod, or rather an indefinite number of infinitely near equal platinum plates. Consider them to form equal branches of a divided circuit, and suppose the strength of current the same in each.

Suppose the number of molecules of hydrogen ejected from any layer in a given time to be proportional to the total number in that layer. Let $u_x$ be the quantity of hydrogen in a layer at distance $x$ from A; then, when equilibrium is attained, $\Delta^2 u_{x-1}$ is proportional to the number of molecules destroyed in that layer in a given time; hence it is 0 between A and B and between C and D, but it is constant between B and C; or the curve representing quantity of hydrogen is straight, except between B and C, where it is conic. I assumed that there is no discontinuity, and that the electromotive force is proportional to the mean quantity of hydrogen in any layer between B and C, or, what is the same thing, to the total quantity between those limits, and that the current is proportional to the total quantity destroyed per second. Hence I deduced the formula

$$(1+na)\mathrm{C}=b-(c+nd)\mathrm{E};$$

where $a$, $b$, $c$, $d$ are constants depending on the lengths BC, CD, on the rate of escape of gas at D &c. The layer A was assumed to be always saturated. In the actual experiment the shape of the plates was by no means regular; and even had they been quite regular, the assumption that the whole of each horizontal layer is a uniform solution is far from the truth. So I wrote the formula in a little more general form,

$$(1+na)\mathrm{C}=b+ne-(c+nd)\mathrm{E}.$$

If in this we put $a=\cdot0006$, $b=244\cdot5$, $e=-3\cdot2$, $c=\cdot00725$, $d=-\cdot0000715$, we get the last column (called "C calc.") given above. We should expect the last two results to be higher than the calculated values, since no allowance has been made for the capillary film rising round the emerging platinum. How-

ever, the formula evidently cannot hold for depths much greater than 63, and it would become necessary to introduce terms varying as $n^2$ &c.

VI. From the above Table it is clear that the electromotive force is not constant, as in ordinary voltaic cells, but rises with the resistance. The same thing happens with ordinary gas-couples with platinized platinum. In one case, changing the external resistance from 46 to 10,000 only lowered the current from 423 to 157 ; in another case, changing resistance from 10,000 to 190,000 lowered the current from 690 to 140.

When the resistance is suddenly increased the strength of current suddenly falls, but it rises, at first quickly and afterwards more slowly, to near its former value. For when the resistance is increased the current falls by Ohm's law ; but it now uses up less gas, so that the gas accumulates in the liquid, and by so doing raises the electromotive force, and therefore the current ; and this continues until equilibrium is attained.

So when the resistance in circuit is diminished the current rises suddenly, but afterwards falls to near its former value. For the current rises by Ohm's law ; but the increased current uses up more gas, and so impoverishes the liquid surrounding the platinum, thereby diminishing the electromotive force, and the current falls.

These observations seem fatal to the hypothesis that the action occurs at the junction of liquid, gas, and platinum ; for the gas at that point remains of constant density, whatever the resistance in circuit may be.

VII. As an example of these views, an ordinary gas element with platinized plates was joined through a resistance of about 10 ohms, including a galvanometer. After about $1\frac{1}{2}$ hour the galvanometer was deflected 195 divisions ; and after 19 hours the deflection was 189. The gas-element was now slanted at an angle of 40°, the plates forming inclined planes ; the current rose gradually, and after $2\frac{1}{2}$ hours the deflection was 235, after $23\frac{1}{2}$ hours it was 221. The element was now rotated 90°, so that the plates were vertical, but their long diameter was still inclined at 40° to the horizon ; the current rose instantly to 265, and after $4\frac{1}{2}$ hours the deflection was 262.

When the plates form inclined planes the line of junction between liquid, gas, and platinum is not altered ; but the whole

surface of the liquid is increased, and the submerged plate is brought nearer to it; hence the current is increased. In the last position an increased line of junction is added, and the current is still greater.

VIII. The current produced by the ordinary gas elements which I used was always greater when the tubes contained but little gas than when they were full of gas, the ratio being, in three cases, $1\frac{3}{4}$, 7, and 18. This is because the greater the distance between the surface of the liquid in the tubes and the air the purer will the solution of gas near that surface be. Perhaps also the greater length of the plate may enable it to catch gas that would otherwise escape: the internal resistance between the most active parts, those near the surface, would be rather increased than diminished.

However, the cells are not at all regular in their action; and this may be due to irregularities in the deposition of the finely divided platinum on the plates. These irregularities do not affect VII., since the tendency of the cell on that occasion was to become gradually weaker.

IX. M. Gaugain found that the electromotive force of platinum-wire electrodes partly exposed to the gas was not altered by submerging them. I have said why this does not appear to me conclusive (5). But I inverted the experiment: ignited wires were put several centimetres below the surface of the gases; the electromotive force was 102. They were then raised so as to be only just submerged; the force was 134. They were then thrust up into the gases, and the force was 136. A key connecting the wires through a galvanometer was pressed down until the needle had got to the end of its first swing; when the needle had come to rest the operation was repeated; and the mean of the two swings is the number given above. The result agrees with M. Gaugain's experiment.

X. On another occasion I measured the electromotive force of wholly submerged wires in a gas-couple by connecting them to a condenser, and afterwards discharging the condenser through a galvanometer. The electromotive force of thick and thin platinum wires was the same; but this was 15 times that of a wire of gold. Probably in the gas-couple, as elsewhere, platinum exerts some specific attraction on hydrogen.

XI. M. Gaugain considers the falling-off in the strength of

a gas-couple after short-circuiting to be due to the deposition of hydrogen on the positive wire, which hydrogen is produced by the decomposition of water by hydrogen; and he says that when the electromotive force of a couple fell from 152 to 30, that of the hydrogen-wire fell 26, while an antagonistic force of 96 was developed by the wire in oxygen*. From other experiments of M. Gaugain, I infer that the potential of each wire was compared with that of a third wire plunged in the liquid between the two tubes of the couple. He does not distinctly say that the positive wire of the couple actually became negative to the third wire, though this may perhaps be inferred from the expression "antagonistic." I consider the loss of potential to be due to the liquid near the wires becoming impoverished of gas; and even should the oxygen-wire become negative to the third wire, it may only show that the liquid in its neighbourhood contains less oxygen than that surrounding the third wire. But since a little hydrogen must have found its way into the oxygen-tube, this has a much better chance of becoming attached to the platinum when there is little oxygen near to use it up (that is, when a current is passing) than when the circuit has been broken and the wire is surrounded by a strong solution of oxygen. Using a gas-couple with wholly submerged platinum wires, and comparing these with a third wire in the liquid between the tubes by means of a condenser periodically discharged through a galvanometer, I found in two different cases, *a* and *b*, just before short-circuiting:—

| | *a*. | *b*. |
|---|---|---|
| Hydrogen-wire ..... | 108 | 74 |
| Oxygen-wire ..... | 12 | 17 |
| | 120 | 91 |

and in the same soon after breaking the circuit:—

| | *a*. | *b*. |
|---|---|---|
| Hydrogen-wire ..... | 41 | 12 |
| Oxygen-wire ..... | 0 | 12 |
| | 41 | 24 |

In one case the oxygen-wire gave a negative deflection of about 1.

* *Comptes Rendus*, 1867.

I found that when I ignited a platinum wire in a Bunsen's flame it acquired a positive potential of about 20, as if it had absorbed oxygen. In §§ IX., X., and XI. the cell was composed of a couple of test-tubes inverted in a beaker of acid, and the wires were introduced by pushing them through a couple of narrow U-tubes, the shorter arms of which were inside the test-tubes. This arrangement made it very easy to change the wires.

XII. The maximum polarization of a voltameter is scarcely, if at all, altered by diminution of pressure (Crova); and the same seems to hold for increased pressure. So also is the electromotive force of a freshly charged gas-couple—being, in fact, little less than that maximum polarization. I connected the terminals of a gas-couple with a condenser which could be discharged through a galvanometer; I then developed gas by electrolysis for a few minutes, during which time the difference of potential between the wires, which I will call E, was 189, the pressure being 77 centims. of mercury; the battery was then cut out, and as soon as most of the bubbles, except those sticking to the wires, had risen, I found $E=60$. The wires were now short-circuited until $E=34$, when the pressure was increased to 145 centims., after which the battery was put on and $E=191$; then the battery was cut out as before, and $E=62$; then the wires were short-circuited till $E=37$; then the pressure was reduced to 22 centims.; then the battery was put on, and $E=200$; after cutting out the battery, $E=60$. The initial electromotive force of the element is not affected by the length of time the battery is in circuit.

The difference of potential between the terminals of the battery was about 260, but was slowly rising during the experiment. When hydrogen is liberated from the surface of the wire, the platinum attracts as much of it as it can: this quantity seems not to vary with the pressure; I do not know why it should so vary; and it determines the maximum polarization. The slight increase of the polarization with pressure may perhaps be attributed to changes in the density of the oxygen.

XIII. A gas-couple charged with chemically prepared oxygen and hydrogen was short-circuited through a galvanometer of 123 ohms resistance, and subjected to various pressures, the top of

the wire in hydrogen being 5·7 centims. below the surface of the liquid in its tube, and that of the wire in oxygen being 5·4 centims. below the surface. At a pressure of 76 centims., deflection 15. At a pressure of 144 centims. the deflection gradually rose in 7 hours from 12 to 26. The hydrogen-wire being 2·1, and the oxygen wire 1·9 centim. below the surface,

at a pressure of 76 the current is 38,
" " 40 " " 20 after 6 hours,
" " 16 " " $7\frac{1}{4}$ after 5 hours.

Now pressure divided by current for the last three cases gives 2, 2, 2·2 respectively; or the current is directly as the pressure. In the last case $7\frac{1}{4}$ seems to be somewhat too low; but this may be attributed to bubbles of oxygen, which under the low pressure were given off in the hydrogen tube. For the first two cases, pressure divided by current gives 5·1 and 5·5 respectively. It is possible that at the end of the second experiment the current was very slowly rising: the further the wires are from the gas, the longer, of course, does it take for equilibrium to be attained.

In this experiment the gases were introduced by stopcocks into the upper parts of the branches of a U-tube, the platinum wires were sealed into the lower parts of those branches, and the bend of the tube had a tail by which the pressure was applied; so that the gases were introduced without coming into contact with the wires. The same instrument, being at hand, was used in § XII., where it is called a voltameter.

If the action of the gas-couple depends entirely on solution, it is natural that the current should be proportional to the solubility of the hydrogen—that is, to the pressure. But if there is really any antagonistic force kept up by hydrogen attached to the positive wire, we should expect that this force would not be altered by pressure, and so the whole current could not be proportional to the pressure. I suppose that when, by increased pressure, the electromotive force becomes equal to the maximum polarization, further increase of pressure would not alter the current.

The Physical Laboratory,
University College, London,
December 1877.

## XXIV. *On some Physical Points connected with the Telephone.*

*By* William Henry Preece, *Vice-President of the Society of Telegraph Engineers, Memb. Inst. C.E., &c.**

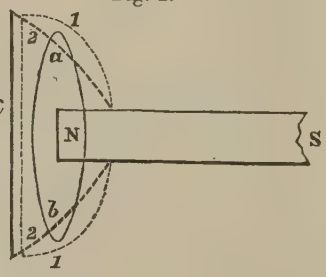
The introduction of the speaking telephone, by Alexander Graham Bell, has supplied physicists with an instrument of research as well as with an instrument of practical utility. It is an apparatus which, for the examination of certain kinds of currents of electricity, is the most delicate that has yet been invented. Indeed it has rendered evident the presence of currents whose existence, though suspected, have hitherto eluded the grasp of the electrician; in fact its very delicacy has proved the greatest obstacle to its general adoption.

### I. *The Telephone as a Source of Electricity.*

Faraday showed that, when a closed conductor is moved across the lines of force in a magnetic field, a current of electricity is generated within that conductor whose strength is dependent upon the velocity of motion of the conductor and upon the intensity of the magnetic field. It is, in fact, proportional to the number of lines of force cut through per unit of time. And also, when lines of force are projected through a closed conductor, a current of electricity is generated in that conductor, whose strength depends upon the magnetic intensity of those lines of force, or upon their number per unit area. The direction of the current in each case is found by Lenz's law, viz. that the current produced tends to resist the motion producing it. The new principle that has been developed by Professor Graham Bell is that the form and duration of that current is dependent upon the rate and duration of the motion of the moving body or of those lines of force.

Let N S, fig. 1, be a permanent magnet, and *a b* a fixed, closed, conducting ring of copper around one pole of that magnet. Let *c* be a movable iron armature. Now, if we regard any two lines of force 1 radiating from the pole N, and nearly cutting the ring *a b*, then, as we make *c* approach or recede

Fig. 1.




* Read January 19, 1878.

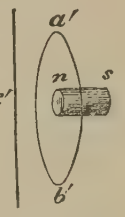
from N, those lines of magnetic force will change their direction, taking up position 2; and with each change of direction they will cut the ring *a b*, and currents of electricity in different directions will circulate through *a b* according to the direction of motion of the lines of force; and the rate of increase and decrease of magnetic intensity (or of the increment and decrement of the current) will vary directly with the rate of motion of the armature *c* to or from the pole N. Thus, if *c* be a disk of iron vibrating under the influence of sound, the excursions to and fro of any point of the disk, though very small (in fact they are so small that they can scarcely be detected by the most delicate means—so small that they have led Graham Bell to imagine that the vibrations are molecular), are nevertheless sufficient to produce that motion of the lines of force which results in currents. It is, however, a fundamental principle in acoustics, that wherever there is sound there is always vibration. Sound and vibration are concomitant and inseparable phenomena. The air cannot produce sound unless it is thrown into vibration; and the air itself cannot be thrown into vibration unless the mass of matter in contact with it vibrates also. The amplitudes of vibration of the particles of the air themselves have never been measured, though the length of a sound-wave (a very different quantity) is accurately known. Lord Rayleigh has shown that an amplitude of only $\frac{1}{10000000}$ of a centimetre is sufficient to produce sonorous vibrations. But though the amplitude of the vibrations be so small they are rapid. Now this rate of motion is sufficient to bend in the same ratio the lines of force cutting *a b*, and thereby to produce currents of electricity in the ring *a b* whose number depends on the number of vibrations, and whose form and intensity depend on the rate and amplitude of motion of the disk *c*. These currents are alternate, and so rapid that no known instrument but the telephone indicates them; but they are readily shown by a Thomson's reflecting galvanometer when the disk is gently and slowly pressed in by the finger,—in one direction when the disk is pressed in, in the other direction when the disk is allowed to fly back.

I have failed hitherto to make even an approximate measurement of their minuteness. We have no known standard to compare them with: we can only trust to the ear; and that

instrument is not only deceptive but variable. They are certainly less than $\frac{1}{1000000}$ of an ordinary working current. Mr. R. S. Brough, of the Indian Government Telegraph Department, has calculated that the strongest current with which a telephone is at any moment worked does not exceed $\frac{1}{1000000000}$ of the C. G. S. unit, or *weber*; and Professor Pierce, of Boston, found that similar effects are produced with an electromotive force of less than $\frac{1}{200000}$ of a volt or Daniell's cell. Thus we have a source of electricity competent to produce currents of microscopic strength, which vary in form, duration, and intensity with the motion of the body producing them.

## II. *The Telephone as a Detector.*

Fig. 2.




Let $n\ s$, fig. 2, be a core of soft iron surrounded by a closed conductor $a'\ b'$, through which currents flow. Now this core will become magnetized with an intensity dependent solely upon the intensity of the current; and the intensity of magnetism at any moment will be a function of the intensity of the current at that moment; so that if the current increase and decrease with a given ratio and at a given rate, the intensity of magnetism will increase and decrease with the same ratio and at the same rate. The disk $c'$ is elastic, but it is rigidly fixed at its axis; it being of iron, it is attracted at any moment with a force dependent upon the intensity of the magnetism of the core $n\ s$, and being elastic, it recovers, or tends to recover, its normal position whenever this intensity of magnetism ceases or diminishes. Thus, if the magnetic intensity varies, the force of attraction varies, and the rate of motion of the disk varies in the same way. Hence the disk will record exactly the variations of the currents; and as the currents are the result of the variations of the vibrations of another disk, the one disk $c'$ simply repeats exactly the vibrations of the other disk: thus sounds are reproduced.

Though in the earlier instruments the coil surrounded a pole-piece of soft iron, this pole-piece has since been discarded, and the coil surrounds the pole of the magnet itself. The efficacy of the instrument has been in no way impaired by this change; and it has the additional advantage of being perfectly

reversible, the same instrument being used for speaking and for hearing.

### III. *Working the Telephone.*

There is a remarkable difference in the power of different voices to work the telephone. Shouting is of no use. The intonation must be clear and the articulation distinct, and the style of conversation approach more the sing-song. I have heard Mr. Willmot, one of the electricians of the Post Office, through resistances that have drowned all other voices. The vowel sounds always come out the best; the palatal sounds *c*, *g*, *j*, *k* and *q*, the worst; in fact, the latter sounds are frequently lost. The ear also requires a certain education; and the power of hearing varies surprisingly with the different ears and with different people. Singing always comes through with remarkable distinctness; and the sounds of a wind-instrument—the cornet or the bugle—are reproduced with startling force. A bugle sounded in London was heard distinctly over the large Corn Exchange of Basingstoke by a thousand people. This arises from the regularity as well as increased amplitude of the sonorous vibrations, and consequently from the regularity, uniformity, and increased strength of the currents of electricity.

### IV. *Improvements.*

Every one who has the means at his disposal has been endeavouring to increase the power of this instrument. I should be sorry to enumerate the number of experiments I have tried, but all with vexatious, disappointing, and dispiriting failure.

One of the earliest efforts was made by Mr. Willmot, who hoped by increasing the number of diaphragms, coils, and magnets acted upon simultaneously, and joining up all those coils in series, to obtain a resultant effect that would magnify the out-going currents; but the result showed that, while the apparatus acted all right, the effect of displacement of each diaphragm decreased with their number, and the ultimate effect was the same as with one diaphragm. Mr. Willmot's instrument, which was made early in October last, is on the table; M. Trouvé, in Paris, seems to have been working on the same idea.

Increasing or varying the size, form, and strength of the magnet has produced little or no apparent improvement; for the resultant effect in all cases remained apparently the same.

The greatest effect is produced with a compound horseshoe magnet, which is indeed one of the earliest forms brought out by Mr. Bell. Here we have two coils, utilizing the maximum number of lines of force; and the effects produced are certainly the finest I have yet experienced. At Southampton, on the 14th inst., in a small office, Mr. Willmot's voice (he was in London) was heard distinctly by the seven or eight persons who were in the room at the time. Though I have made one with the largest and most powerful magnet I could obtain, the result has been as disappointing as in the previous cases. The telephone has certainly been brought to this country by Mr. Bell in almost its perfect theoretical form; he is still labouring to improve it; and I am sure we all wish him success.

## V. *Applications.*

However small and however sudden the currents may be, the telephone records them with great accuracy; no known form of galvanometer or galvanoscope will do so.

It is admirably adapted for showing the currents of induction set up in contiguous coils or contiguous spirals. If reversals or intermittent currents be sent through one spiral while the other be gradually removed away, the rapidly diminishing effect of increased distance is very evident; indeed all the phenomena of magneto-electric induction are strikingly shown by its means. It is also admirably adapted as a detector in the bridge of a Wheatstone's balance to test short lengths of wire, and it will probably enable us to obtain a closer approximation to equality than we have yet secured; it also enables us to adjust condensers with great accuracy.

M. Niaudet, of Paris, has shown how it can be utilized to detect the presence of extremely feeble currents from doubtful sources of electricity. If currents from the supposed source be rapidly sent through one wire of a double-wound coil, and a telephone be fixed on the other running parallel to it, then the telephone would give evidence of their presence, which would be indiscernible on any other instrument.

It is admirably adapted also for testing leaky insulators and supports.

### VI. *Inferences and Results.*

The telephone explodes the notion that iron takes time to magnetize and time to demagnetize. If time were occupied in magnetizing, notes would be changed or lost; but they are not altered. The notion of time is due to the action of induction in coils producing *reaction* and *extra currents*. This is proved by the insertion of an electro-magnet or of coils of wire in a telephonic circuit. While it is possible to speak through a cable 100 miles long laid out straight in the sea, it is impossible to speak through 20 miles when coiled in a tank.

Its delicacy has detected the presence of currents in wires contiguous to wires conveying currents, which have always been suspected, but have been evident only on wires running side by side for several miles (say two hundred) on poles or in well insulated cables. In fact, the most delicate apparatus has hitherto failed to detect the presence of these currents by induction in short underground wires; but the telephone responds to these currents when the wires run parallel for only a few feet. Thus, between one floor and another floor, at the General Post Office, it has been impossible to converse by means of the telephone through a wire, owing to the presence of these currents of induction from the innumerable working wires contiguous to it, and through some of the underground pipes of the streets of London sounds are inaudible when the wires are working. In fact, two small-sized gutta-percha wires, *one foot long*, were lashed side by side by Mr. Marson; and when battery currents were sent through one, induction currents were distinctly heard on a telephone fixed on the other. Indeed this induction between wire and wire, has proved the most serious obstacle to the practical introduction of the instrument. But it is not altogether irremediable on underground wires; it can be surmounted in three ways:—

1. By increasing the intensity of the transmitted currents so as to overpower the currents of induction, and by reducing the sensitiveness of the receiving apparatus so as to make the instrument insensible to currents of induction though responsive to telephonic currents.

2. By screening the wire from the influence of induction.

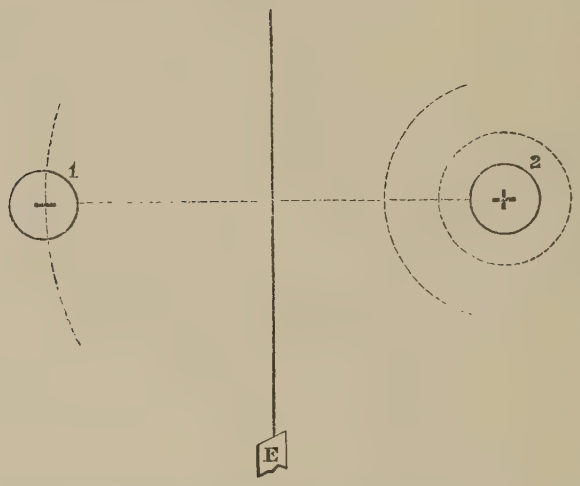
3. By neutralizing the effects of induction.

1. Mr. Edison in America has partially succeeded in effecting the first cure ; but his results, though promising, have not yet reached a practical point.

2. I have overcome the second difficulty in a way that will now be described.

Let 1, fig. 3, be a wire used for telephonic purposes, and 2

Fig. 3



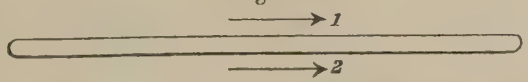

be an ordinary telegraphic wire contiguous to it. Let us regard 1 and 2 as symmetrical and contiguous particles of the two wires. If a current flow through 2 it will affect 1 inductively both statically and magnetically. Let us regard the static effect first. If the current flow away from us, then we may consider the particle 2 as charged *positively* ; lines of electric force will radiate all around it, and that line which passes through 1 will inductively charge that particle *negatively*. This influence being felt all along the wire, a current in the reverse direction to that in 2 will flow through 1. The reverse would occur if we assumed the primary current to flow in the other direction. Hence, an induced current will flow through 1, whenever the current in 2 commences and whenever it ceases. Now, if we place between 1 and 2 a screen of metal, or other conducting matter, in connexion with the earth, then

the line of electric force, instead of passing through 1, will terminate at the screen. Hence, if we surround the wire 2 with a covering or sheath of metal, or if we submerge it in water, all effects of static induction will cease between 1 and 2. In water they are not entirely eliminated, for water is a very poor conductor; but they are so reduced by its influence, as my experiments between Manchester and Liverpool and between Dublin and Holyhead have shown, that, if the water or wet serving had been a perfect conductor, they would have been removed as far as regards static induction.

But we have to regard magnetic induction as well. Besides establishing a field of electric force around 2, a current flowing through that wire establishes a magnetic field around it, whose lines of force are circles, and whose directions are at right angles to the lines of electric force. Let us regard that line of force cutting 1. Each time a current commences, and each time it ceases, in wire 2, a line of magnetic force cuts wire 1, and produces in that wire a current of induction in the same direction as that produced by static induction. Now, if we make the screen of iron, those lines of force terminate in the iron and wire 1 is freed. Hence, if we sheath the wire 1 with iron, it is not only freed from the effects of static induction by being surrounded by a conductor in contact with the earth, but it is shielded from the effects of magnetic induction by its sheath of iron. Hence both effects of induction are entirely removed.

3. They can be neutralized by means of a return wire, using this return wire instead of the earth. If 1 and 2, fig. 4, be

Fig. 4.




two wires running side by side, then the current set up by induction from neighbouring wires in one wire is neutralized by the currents set up in the other side.

But this assumes either that the disturbing wires are at an infinite distance from 1 and 2, or that 1 and 2 are infinitely near each other. All attempts to use return wires on existing poles, in cables, or in underground wires have utterly failed to do away with inductive disturbance; but Mr. Bell has had a

single gutta-percha wire carrying two conductors made which very nearly fulfils the conditions and gives excellent results.

The extreme delicacy of the instrument has introduced a disturbance from another cause, viz. leakage. Wires on poles are supported by glass, porcelain, and earthenware insulators; but the best support that was ever devised is but a poor insulator in wet weather. Currents escape over their surface from the wire they support; and these leakage currents find their way into telephonic circuits. Hence a telephone circuit which may work well in dry fine weather will prove absolutely unworkable in wet weather.

Another source of trouble arises from what are technically called "bad earths." It is almost impossible to make a perfect connexion with the earth. There is always some resistance at that point; so that if two wires terminate on the same earth-plate, the one being a working circuit and the other a telephone-circuit, some currents from the former are sure to pass through the latter and disturb the telephone. A return wire perfectly cures this evil.

There are other disturbing elements that are peculiar. Earth-currents, which are always present in the wires, produce a peculiar crackling noise, similar to that produced by a current from a single fluid battery such as a Smee or a Leclanché, not unlike the rushing of broken water. This is due to the polarization of the earth-plate, as the sounds produced by a battery-current are due to the polarization of the negative plate. When auroras are present these earth-currents become very powerful, and the sounds are much intensified. The effects of thunderstorms are very peculiar: a flash of lightning, even though so distant as to be out of sight, will produce a sound; and if it be near enough to be only sheet lightning, it produces, according to Dr. Channing, of Providence, a sound like the quenching of a drop of melted metal in water, or the sound of a distant rocket. Moreover he says that this sound is heard before the flash is seen, proving the existence of some inductive effect in the air prior to the actual discharge. The telephone thus becomes an admirable warning of the approach of a thunderstorm.

Sometimes a peculiar wailing sound is heard, which an imaginative correspondent of mine likened to "the hungry

cry of newly-hatched birds in a nest." I am inclined to think that it is due to the swinging of the wires across the magnetic lines of force of the earth. It is not difficult to conceive that these vibrations may succeed each other in the necessary rhythmic order to produce musical tones.

The wires are never free from sound; and every change of temperature or of the electric condition of the atmosphere is recorded on this delicate apparatus.

The expansion of the iron diaphragm under the influence of the warm and damp breath when the telephone is first raised to the lips preparatory to talk is very marked; it produces a faint rustling shiver.

Immediately on the introduction of the instrument, great anxiety was felt to learn its performance on submarine cables. A telephone was sent to Guernsey, and Mr. Willmot went to Dartmouth, those two places being connected by a cable 60 miles long. Conversation was carried on, the articulation being perfect though slightly muffled. This was a surprise; for it was felt that the static induction of a cable, by its retarding influence, would have prevented articulation by lengthening the waves of electricity and rolling them up as it were. Through the kindness of Messrs. Latimer Clark, Muirhead, and Co. I was able to repeat these experiments on an artificial Atlantic cable, constructed to duplex the direct United-States cable. With Mr. Willmot at one end and myself at the other, there was no difficulty in speaking up to 100 miles, though the muffling effect of induction was evident. Beyond this distance up to 150 miles muffling commenced to seriously impede conversation, and the sounds diminished considerably in strength: it was like talking through a thick respirator. The effect diminished rapidly up to 200 miles, beyond which articulation became impossible, though singing was distinctly heard; indeed singing was heard through the whole length of the cable, 3000 miles long; but this was traced to a secondary cause, it being due to the induction of condenser on condenser. Nevertheless there is no doubt that singing can be heard through a much greater length than speaking, due to the greater regularity of the successive waves of electricity.

I subsequently experimented on the underground wires between Manchester and Liverpool, a distance of about 30 miles;

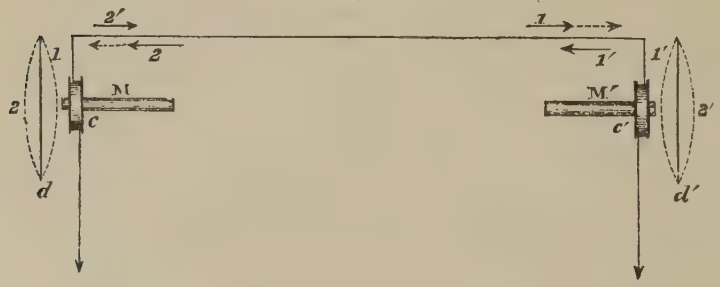
and through this length we had no difficulty whatever in speaking. Again, between Dublin and Holyhead, through the cable 67 miles long, we spoke with ease, singing coming through with remarkable power and effect. This cable contains 7 distinct conductors. When one wire was used for the telephone, the sounds could be heard on every other wire, but in a feebler degree. When the other wires were working with the ordinary telegraphic apparatus, induction was evident, but not sufficiently intense to stop conversation. Each wire would be surrounded with a wet serving of hemp; but this was not of sufficient conducting-power to entirely screen the effect of induction. The same effect was experienced between Manchester and Liverpool, where the wires are made up into cables of 7 conductors served outside with tarred hemp.

The conclusion that I have come to is, that conversation might be held through a single wire cable 200 miles long with the apparatus that we now have; what new apparatus will do no man is rash enough to predict.

The reason for this surprising result is not difficult to explain:—

Let the disk $d$, fig. 5, be impressed forwards by a sonorous vibration, it will generate in the coil $c$ a positive current,

Fig. 5.




which, flowing through the line, will pass through the coil $c'$ and attract the disk $d'$. Now the effect of induction is to retard or prolong the effect of the positive current 1; but the motion of the disk $d'$ has itself produced a current in the reverse direction to the first current; and this neutralizes the prolongation due to induction, and so helps to clear the line

for the next signal, which passes through precisely the same process; and hence the vibrations of the second disk tend to produce currents which diminish materially the effects of induction, and so render possible conversation to distances that far exceed anticipation.

The extreme delicacy of Bell's apparatus has been shown in various ways; for instance :—

*Extract from a letter from* T. A. EDISON, *dated November 25, 1877.*

"I made a pair of telephones that work with copper diaphragms: it is on the revolving-copper-disk principle of Arago.

"I find that a copper diaphragm may replace the iron in Bell's. Copper must be $\frac{1}{32}$ in. thick. It is very low with copper in both; but if the receiver is one of the regular kind, and the transmitter is a copper diaphragm, you can carry on conversation with ease both ways; but with the pair I have made the talking is loud, as I have several dodges on it."

I repeated these experiments; but the effect was so feeble as to be scarcely distinguishable, and, although interesting from a scientific point of view, it was of no practical value.

Mr. James Blyth has independently repeated the experiment, and has shown that wood, paper, and india-rubber produce similar effects. These effects are probably due to the fact that diamagnetic bodies have a similar though feebler influence, in varying the direction of lines of magnetic force, to magnetic bodies.

Again, I have spoken distinctly and easily with telephones without any permanent magnet whatever, the core of the coil being of soft iron; but this effect was probably due to the impurity of the iron, residual magnetism remaining in it. Dr. Blake, of Boston, has spoken easily when the core was a piece of soft iron placed in the direction of the dip.

XXV. *The Production of Thermoelectric Currents in Wires subjected to Mechanical Strain.* *By* G. W. von Tunzelmann, *Holder of the Clothworkers' Exhibition in Chemistry and Physics at University College, London.*

The following inquiry was suggested by some observations recorded in a paper of Sir William Thomson's on the Electrodynamic Qualities of Metals, in the 'Philosophical Transactions' for 1856; and the object in view was to investigate the conditions under which thermoelectric currents are produced in a circuit composed of a single metal when one portion of the metallic conductor is subjected to a strain and the junctions of the strained and unstrained portions are maintained at different temperatures.

The experiments were made upon wires of iron, steel, and copper, the copper wire employed having been obtained from Messrs. Johnson, Matthey & Co. as chemically pure.

Two tin cans were obtained open at the top, and pierced at the bottom by necks into which india-rubber corks were inserted; and through slits in these the wires were passed. The wire was fastened by a clamp in the lower can, and was grasped in the upper one by a pair of wire-drawing dogs attached to the shorter arm of a lever, to the longer arm of which was attached the weight by which the strain was produced. In the earlier experiments ordinary weights were used; but ultimately these were rejected, as it was found impossible to apply and remove them in a sufficiently gradual manner to prevent a certain amount of shock, which introduced complications.

In the arrangement finally adopted, there was attached to the longer arm of the lever a tin can open at the top, and having at the bottom a neck fitted with an india-rubber tube, which could be closed by merely bending it up and hitching it in a hook attached to the can for that purpose. The strain on the wire was then produced as gradually as was desired, by pouring in measured quantities of shot; and it could be removed as gradually by letting the shot run out by the india-rubber tube.

The two cans through which the wire passed were filled with water, the water in the upper can being kept at the temperature of 100° C. by means of a gas-burner, while that in the lower can could be kept for a considerable time at a uniform temperature by allowing a current of water, of the same temperature as the place of experiment, to circulate through it.

The extremities of the experimental wire were bent round in a large curve and brought close together; they were then tied to the extremities of two covered copper wires connected, through a four-way key, with a Thomson's galvanometer having a resistance of between one and two ohms. The junctions were then placed side by side separated by thin paper, and wrapped up in cotton-wool, as was done by Thomson in his experiments, to prevent the production of currents by the unequal heating of the two junctions.

Before being used the wires were annealed:—the iron and steel wires by being heated to redness in an iron tube, through which a current of coal-gas was passing to prevent oxidation; the copper wire by being slowly passed through a Bunsen flame, as it was found that the exposure of the copper at a red heat to the current of coal-gas produced an effect similar to that known as over-poling in the process of refining copper, the wire being rendered so brittle as to break with the least strain.

Thomson found in his experiments that when a weight was applied so as to produce a state of strain in a portion of the wire, and the two junctions of strained and unstrained portions were kept at different temperatures, in iron and steel wire a current was produced the direction of which was from the unstrained to the strained portion across the hot junction, while in copper wire the current was in the opposite direction. When the weight was removed the result was in either case a weaker current in the reverse direction.

Some experiments of the same nature have also been made by M. le Roux, and described in the *Annales de Chimie et de Physique*, 4th series, vol. x. p. 201 (1867). He obtained results of the same nature as Thomson—with the notable difference that in iron and steel wires he got a current from strained to unstrained across the hot junction, while in copper wire the current was from unstrained to strained across the

hot junction. On comparing the descriptions of the experiments, it appeared that Thomson had always made his experiments with comparatively small strains, while Le Roux had strained his wires very nearly to the breaking limit. This at once suggested a possible explanation of the apparent discrepancy between their results; and on making the experiments, it was found that as the strain was gradually increased the current was increased, as in Thomson's experiments, but only up to a certain limit. When the strain was increased beyond this limit there was a gradual decrease in the current; and if the strain was very carefully increased, the direction of the current was reversed shortly before the breaking strain was reached.

During the experiments, it was very soon observed that after a weight had been added the current did not remain constant, but gradually diminished; while at the same time there were variations of small period in the strength of the current, which were greater when the weight was added suddenly, and scarcely perceptible when it was very carefully and slowly added by pouring in shot; these variations gradually ceased when the apparatus was not disturbed. A very gentle and gradual addition of weight diminished these variations, which always died away more rapidly when there was a heavy strain on the wire. Clutching the wire in the "dogs" also set up these variations, which were allowed to subside before beginning the experiment. These results suggested that the production of the current might be due to a process of change in the molecular state of the wire; it was found, however, on examination that there was a permanent effect which could not, as far as I can see, be produced in that way, whatever may have caused the temporary effect.

Where the results obtained at different times had not to be compared, the current is generally given in terms of the deflections of the galvanometer; but where such comparison was necessary, the value of the deflections was determined at each experiment in terms of a standard current obtained by sending a current from a Daniell's cell through a definite resistance.

For the sake of brevity, U.S. will be written for "from unstrained to strained across the hot junction:" and the oppo-

site direction of the current will be denoted by S.U. The following letters are used in the description of the experiments:—

W = tension applied to wire in pounds, = 3 times weight actually applied to lever;
M = number of measures of shot effective in stretching wire, = 3 times number actually applied to lever;
$\Theta$ = temperature of lower can;
D = mean deflection of galvanometer;
C = strength of current in terms of the standard current.

A considerable number of preliminary experiments were made to verify Thomson's results and to determine the best form of apparatus, the arrangement ultimately adopted being that already described. These experiments (which are not described here) gave a general idea of the phenomena to be looked for. The alteration of resistance from strain is not taken into consideration, as H. Tomlinson's experiments, Proc. Roy. Soc. 1876 (vol. xxv. p. 451), have shown that it is too small to have an appreciable influence upon the results.

*Experiment* 1.—An iron wire ·46 millim. diameter. $\Theta = 16°$; W = 31·5. The result is given in the accompanying Table, the direction of the current being U.S., the first reading being taken immediately after the weight was applied. It will be observed that the current does not reach its full strength immediately upon the application of the weight, but rises rapidly to a maximum, and then gradually falls to a strength at which it remains steady.

| Time (minutes). | D. |
|---|---|
| 0 | 2·5 |
| 5 | 6 |
| 10 | 3 |
| 15 | 2 |
| 20 | 2 |
| 25 | 2 |

*Experiment* 2.—A similar wire. $\Theta = 16°$. The readings were taken immediately after the application of the weight. The sign * means that there is a deflection, but too small to be measured.

| W. | D. | Direction. |
|---|---|---|
| 6 | * | U.S. |
| 9 | 5 | U.S. |
| 12 | 3·5 | U.S. |
| 15 | 3 | U.S. |
| 18 | * | S.U. |
| 21 | wire broke. | |

It will be observed that the direction of the current changes just before the wire breaks.

Some steel wire was now taken; and as it was found almost impossible by the most careful annealing to get a piece of wire arranged in the apparatus which should give no deflection before the application of the weight, the initial deflection was noted in each case.

*Experiment* 3.—A steel wire ·81 millim. diameter. $\Theta = 16°$. Initial deflection = 20 S.U. $W = 31{\cdot}5$. After the application of the weight the deflection reversed its direction to U.S. The result is given in the following Table.

| Time (minutes). | D. |
|---|---|
| 2 | 10 |
| 3 | 12 |
| 4 | 13 |
| 5 | 15 |
| 6 | 18 |
| 24 | 10 |
| 25 | 9 |
| 50 | 9 |
| 90 | 5 |
| 130 | 3 |

The apparatus was then left for about 90 hours with the weight attached; $\Theta$ being still 16°, there was now a deflection of 15 S.U. The weight was then removed, and the deflection fell rapidly and changed to U.S. The subsequent deflections were as follows:—

| Time from removal of strain (minutes). | D. U.S. |
|---|---|
| 1 | 2 |
| 2 | 3 |
| 3 | 4 |
| 4 | 6 |
| 5 | 7 |
| 6 | 8 |
| 14 | 8 |
| 20 | 10 |
| 25 | 7 |
| 30 | 3 |

In both cases we observe that the current gradually rises to a maximum and then falls.

*Experiment* 4.—A steel wire ·98 millim. diameter. $\Theta = 15°$. Initial deflection = 10 S.U. On the addition of a weight of 42 lbs., the deflection changed rapidly to 14 U.S., and in about an hour fell to 8 U.S., and in 40 hours to 3 U.S.

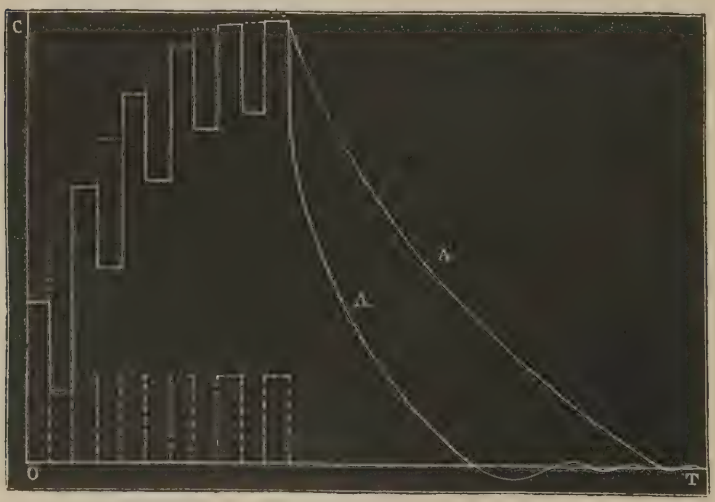
*Experiment* 5.—A steel wire ·6 millim. diameter. $\Theta = 13°$. Initial deflection = about 1 U.S. W = 63 lbs. Direction of current after application of weight U.S. The weight was now left suspended for about 40 hours; but the apparatus received an accidental jar before the permanent deflection could be ascertained. The weight was then removed, causing a deflection of 8 S.U., gradually increasing to 11, and then decreasing much more slowly than it had increased.

| Time from removal of strain (minutes). | D. |
|---|---|
| 3 | 3·5 |
| 6 | 4·5 |
| 9 | 3·5 |
| 12 | 2·5 |
| 15 | 1·5 |
| 18 | ·87 |
| 36 | * |

The effect of rapidly putting on and taking off the weight a number of times in succession was then tried; and it was found that each time the weight was put on the deflection diminished, while each time that it was taken off the deflection was increased up to a certain limit. The immediate increase produced by taking off the weight was greater than the immediate decrease produced by putting it on; so that on the whole there was a large increase in the current, the deflection being got

up in this way to nearly 30 S.U., falling again very rapidly if the weight were left attached to the wire. Under these circumstances the deflection went down rapidly to zero, changed sign, rose to a maximum, and then again began to diminish, passed through zero in the opposite direction, and so continued to perform excursions in alternate directions, and very rapidly decreasing in extent. When the weight was permanently removed the deflection of the galvanometer died out much more slowly, and the changes of sign were only just perceptible. These phenomena confirm the conclusion to which I was led by the former experiments, that there is, besides the main effect, a transient effect produced by altering the strain on the wire; and this transient effect appears to me to be due to the molecular state of the wire making a partial return after the first shock towards its primary condition, just as the immediate deflection of a spring suddenly stretched by a weight is greater than when it has come to rest in its position of equilibrium. The changes of sign in the current as it gradually comes to its final state after the wire has been violently disturbed, as in the last experiment, as also the fact of there being a permanent as well as a temporary effect, seem to render this hypothesis more probable than that the current is actually produced by a change in the molecular state of the wire.

The phenomena obtained in the last experiment will be rendered more clear by a diagram.



The intervals between taking off and putting on the weight were approximately equal. These are therefore represented by equal distances along the axis O T; and the strength of the current is set off along the axis O C. Starting from the point O, at the beginning of the experiment, with the weight attached to the wire as it had been left, then at any time the broken line

----- represents the permanent change in the current produced by taking off or putting on the weight;

..... represents the temporary change;

—— represents the resultant strength of the current, being the sum of these two components.

The curve A represents the change in the current when the weight is left permanently attached; and the curve A′ represents the change in the current when the weight is permanently removed.

At this stage in the experiments the method of measuring the current-strength in terms of a standard current was adopted.

The battery used as a standard was a "sawdust" Daniell's (Menotti's) cell; and the strength of the current was approximately that produced by 1 volt through 10,000,000 ohms, or $10^{-8}$ C.G.S. unit. The measurements of the Tables are given in millionths of a C.G.S. unit.

*Experiment* 6.—A similar steel wire. $\Theta = 16°$. Initial deflection barely perceptible S.U. Weight of 30 lbs. left on about 40 hours.

It was observed that at the time of making the experiment the weight was making small oscillations; and this appeared to be the cause of the deflections making small oscillations about a mean value. At the end of an hour and a half the oscillations of the weight and also of the deflection had ceased, the latter remaining steady at 5, indicating a current ·007 U.S. The weight was now made to perform vibrations of small amplitude, upon which the oscillations of the deflection were greatly increased both in number and amplitude, and the mean deflection was at the same time somewhat increased. If the vibration of the weight be suddenly stopped, it is some little time before a decrease is perceived in the oscillations of the deflection.

After setting the weight in gentle vibration, the effect in causing oscillations in the deflection was observable in less

than a minute. If the vibrations of the weight are kept up for some time, the mean deflection is increased up to a certain limit, as before described. If the vibrations of the weight are increased in amplitude, the oscillations of the deflection become much more irregular, and the limits of variation become greater.

*Experiment* 7.—A similar wire. $\Theta = 12$. Initial current ·0014 U.S. A weight of 3 lbs. was now attached; and at the end of two minutes there was a current ·0052 S.U., falling at the end of an hour and a half to ·0034 U.S. The weight was then increased by 3 lbs. at a time and the deflections taken immediately, with the results given in the accompanying Table:—

| W. | C. | Direction. |
|---|---|---|
| 6 | ·0038 | U.S. |
| 9 | ·0019 | U.S. |
| 12 | ·0014 | U.S. |
| 15 | ·0012 | U.S. |
| 18 | ·0012 | U.S. |
| 21 | ·0012 | U.S. |
| 24 | ·0012 | U.S. |
| 27 | ·0010 | U.S. |
| 30 | ·0002 | U.S. |
| 33 | * | S.U. |

In the experiments after this the weights are given in terms of measures of shot, each of which weighed about 7480 grains.

*Experiment* 8.—A similar wire. 33 measures left on for about 40 hours. $\Theta = 12°$. Deflections read immediately after removal of weight. Direction of current S.U. There was no initial deflection.

| M. | C. |
|---|---|
| 33 | ·0095 |
| 30 | ·0046 |
| 27 | ·0035 |
| 28 | ·0039 |
| 21 | ·0049 |
| 18 | ·0060 |
| 15 | ·0067 |
| 12 | ·0074 |
| 9 | ·0084 |
| 6 | ·0091 |
| 3 | ·0098 |
| 0 | ·0105 |

*Experiment* 9.—A similar wire. $\Theta = 12°$. No initial deflection. Deflections read immediately after application of weight.

| M. | C. | Direction. |
|---|---|---|
| 3 | ·0025 | U.S. |
| 6 | ·0025 | U.S. |
| 9 | ·0021 | U.S. |
| 12 | ·0014 | U.S. |
| 15 | ·0014 | U.S. |
| 18 | ·0014 | U.S. |
| 21 | ·0014 | U.S. |
| 24 | ·0014 | U.S. |
| 27 | ·0011 | U.S. |
| 30 | ·0007 | U.S. |
| 33 | ·0004 | S.U. |
| 36 | ·0014 | S.U. |

*Experiment* 10.—A steel wire ·47 millim. diameter. $\Theta = 12°$. Initial deflection 4 U.S. On attaching the empty can for containing the shot to the end of the lever the deflection increased to 20 U.S., falling to 14·5. The strain was gradually increased by pouring shot into the can until the wire broke. The deflection changed very little until the wire began to stretch, when the deflection fell very rapidly, passed through zero, and went up to about 40 S.U.

The more rapid the stretching the stronger is the current produced. When the strain was slightly lessened, so as to stop the stretching, the deflection fell very quickly to 20 S.U. On removing the strain the deflection fell rapidly, passed through zero, and went up to 2 or 3 U.S., making irregular vibrations. The weight was replaced and additional shot poured in very slowly. The deflection almost instantaneously changed to about 2 S.U., which increased slightly until the wire broke.

*Experiment* 11.—A copper wire ·24 millim. diameter. $\Theta = 15°$. Initial deflection 1 S.U.

| M. | D. |
|---|---|
| 0 | 1 |
| 3 | 1 |
| 6 | 3 |
| 9 | 3 |
| 12 | 3 |
| 15 | 4 |
| 18 | 4 |
| 21 | 2·5 |
| 24 | 2·5 |
| 27 | 2 |
| 30 | 1·5 |
| 33 | 1·5 |
| 36 | 1·5 |
| 39 | 1·5 |
| 42 | 1·5 |

The direction of the current was S.U. Several small weights were added to the can; but the deflection remained steady at 1·5. In copper wire, no fall in the deflection was observed when the weight was left suspended for some time.

The Physical Laboratory,
University College, London.

---

## XXVI. *On the Influence of Temperature on the Passage of Air through Capillary Tubes.* *By* Francis Guthrie, *LL.B.**

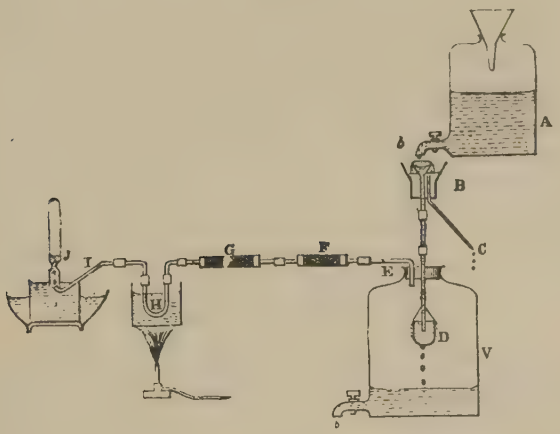
In using "Marsh's" apparatus for testing for arsenic, it may be noticed that, when heat is applied to the exit-tube to decompose the arsenuretted hydrogen, the liquid rises in the tube which supplies the sulphuric acid, thus leading to the supposition that the passage of the gas through the exit-tube is checked by the increase of temperature, thereby producing increased pressure. This observation suggested the following experiments on the effect of heat on the passage of gases through capillary tubes.

The apparatus used was as follows:—From a vessel, A, water drops into a funnel, *b*, causing a continued overflow.

* Communicated to the Physical Society, April 13, 1878. A note of the results was communicated to the British Association, 1876.

The overflow falls into the concentric funnel B, and escapes by the tube C. The middle of the funnel-tube is removable, and

Fig. 1.




may be replaced by tubes of any required lengths. The lower end of the funnel-tube passes air-tight through the cork of a bottle, V, and has hanging from its end a little thimble of glass, D. By this means, whatever amount of water has entered V, the pressure in V is that due to the difference of height of the water in *b* and D. The air from V is dried by two chloride-of-calcium tubes, F and G, and then passes through the experimental capillary tube H, which can be heated. Thence it passes down the tube I and is collected over water in the tube J, which has a marked stricture. The vessel in which J stands is always overflowing.

It was found that the utmost attention was required to keep the air perfectly dry. A joint of caoutchouc in water is found to be porous to water near its boiling-point, the water probably penetrating as a vapour. As the slightest intrusion of water would vitiate the experiment, paraffin was used in such cases.

The first point to be decided on was, whether heating a current affected its rate of motion independently of its friction. At first sight this seemed not to be impossible; the expansion of the air while its temperature is being raised might, it was thought, react on the air behind it and thus check its outflow. Experiments, however, showed that this was not the case, and

that the effect of heat in checking the current is due solely to its influence on gaseous friction.

It was found that the amount of air passing down a given capillary tube varies approximately in the inverse ratio of the square of the absolute temperature, and directly as the difference of pressure at the two ends of the tube.

Neither of these relationships, however, is quite exact; the following formula more nearly expresses actual results. Calling $t$ the time required to fill with air a vessel of given capacity, and T being the absolute temperature reckoned from $-273°$ C., and denoting by $p_1$ and $p_2$ the pressures at the ends of the tube,

$$t = \frac{\alpha T^2}{p_1 - p_2}\left(1 + \frac{\beta}{T(p_1 - p_2)^n}\right),$$

where $\alpha$ is a small fraction depending probably on $p_1$.

The facts that the time varies approximately as the square of the absolute temperature, and is not exactly in inverse proportion to the difference of pressure, are worthy of notice; and their theoretical investigation will throw some light on the molecular theory of gases. The fact that the temperature affects the time according to the square of the absolute temperature is consistent with known molecular laws. It has been shown that the viscosity of air and its consequent shearing friction is in proportion to T. And it is obvious from the fact that the volume of a given mass of air is directly as its absolute temperature, that the time required for the passage of a given quantity of air through a given tube at a given pressure should vary as $T^2$.

More anomalous are the results that the velocity does not exactly follow the simple law that the rates of passage at a given temperature are in proportion to the difference of pressures at the ends, but that the time of passage of a given mass of air is shortened by increased pressure in a somewhat greater proportion than the increase of pressure. The deviation from the law of inverse proportion is only slight, and obviously belongs to a term having a small absolute factor. But an inspection of the results will show that its existence is unmistakable.

To examine the effect of simply raising the temperature, the following experiments were made:—

(1) The current was heated in a wide tube before reaching the capillary tube, being afterwards cooled again to the temperature of the outer air. *Result*:—the time of passage of a given mass of air was slightly increased.

(2) The current was heated after passing the capillary tube. *Result*:– a slight decrease in the time of passage of a given volume. The decrease was only such as might be accounted for from the fact that the air was not completely cooled again.

(3) The heating and cooling of the current in the capillary tube. *Result*:—an increase of time apparently proportional to the length of the tube heated.

The following are the experiments:—

(1) Whole current at atmospheric temperature 22° C.

(*a*) $t=659''$
(*b*) $t=656''$ Mean $657''{\cdot}5$.

The current was then heated to 100° C. in a wide tube before entering the same capillary tube.

(*a*) $t=670''$
(*b*) $t=668''$ Mean $669''$.

The slight increase of time here indicated was probably due to the fact that the air current had not altogether regained its normal temperature before reaching the capillary tube.

(2) Whole current at atmospheric temperature 21° C.

(*a*) $t=157''$
(*b*) $t=156''$ Mean $156''{\cdot}5$.

The current was then heated to 100° C. in a wide tube after passing through the capillary.

(*a*) $t=153''$
$t=155''$ Mean $154''{\cdot}0$.

(3) Capillary tube partly immersed in a paraffin-bath at 200° C. The end cooled to 22° C.

$t=10055''$.

More of the tube being heated,

(*a*) $t=1098''$
(*b*) $t=1094''$ Mean $1096''$.

The effect of heating and cooling of the same current in different tubes in the same circuit was then examined. The

same tubes were timed separately and in conjunction at the same and at different temperatures.

| Tube. | Temperature. | Time. |
|---|---|---|
| 1 | 24°·5 C. | 60″ |
| 2 | „ | 57 |
| 3 | „ | 58 |
| 1 | 100 | 89 |
| 2 | „ | 81 |
| 3 | „ | 82 |
| 1+2 | 24·5 | 117 |
| „ | 100 | 173 |
| 1 | 100 | 203 |
| +2+3 | 21 | |
| 1+2 | 100 | 228 |
| 3 | 21 | |

These results show that the resistance of the sum is very nearly the sum of the resistances, whether all the tubes are at the same temperature or some at one temperature and some at another.

In order to ascertain whether the rate of passage followed any special law at the beginning or end of the tube, the time of the passage of a given mass of air thrust through a given tube at a given pressure and temperature was ascertained; the tube was then divided into parts, and the time of the passage of the same bulk of air through each part was measured.

| | 20° C. | 100° C. |
|---|---|---|
| Entire tube | 372″·5 | 563″ |
| Part 1 | 89·5 | 133·3 |
| „ 2 | 117·5 | 180·0 |
| „ 3 | 63 | 96·5 |
| „ 4 | 101 | 152 |
| „ 1+2 | 208·5 | 315·6 |
| „ 3+4 | 165 | 246·6 |

From this Table it is seen that, within the limits of experimental error, the time taken for the passage of a given bulk of air through the entire tube is equal to the sum of the times taken for the passage of the same amount of air through each of its parts.

A similar experiment with a tube divided into a greater

number of parts was then made. The original tube was about 600 millims. long.

(*a*) $t=85''$
(*b*) $t=83''$ Mean $84''$.

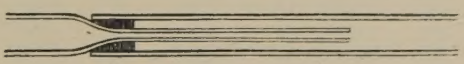
The tube was then divided without loss into 21 parts; these were connected by caoutchouc and covered with paraffin.

(*a*) $t=86''$
(*b*) $t=85''$ Mean $85''{\cdot}5$.

These experiments show that the terminations of the tubes exert no special influence on the passage of the current.

A conical or trumpet-shaped tube was then examined, the arrangement being according to fig. 2. The following were the results.

Fig. 2.



*Wide end towards greatest pressure.*

(*a*) $t=183''$
(*b*) $t=184''$ Mean $183''{\cdot}5$.

*Narrow end towards greatest pressure.*

$t=184''$.

This shows that the rate is the same both ways.

The foregoing experiments having shown that the effect of temperature on the rate of a current was to be looked for in its influence on fluid-friction, the next thing was to determine the relation between temperature and time, other things being the same. For this purpose a capillary tube, through which a current was passing at a constant pressure, was subjected down its entire length to various temperatures—care being taken that the air should arrive at the entrance of the capillary tube at the temperature of the tube itself, so that the current might be of the same temperature throughout the entire length of the capillary tube. The times of passing of a given quantity of air for different temperatures were noted; and some of the results are subjoined.

| Temp. Centigrade. C. | Absolute temp. T. | $T^2$. | Time. $t$. | $\frac{T^2}{t}$. | $\Delta\left(\frac{T^2}{t}\right)$. | $\frac{T}{t}$. | $\frac{T(T+94)}{t}$ |
|---|---|---|---|---|---|---|---|
| **I. Pressure 315 millims. of Water.** | | | | | | | |
| −20 | 253 | 64009 | 119″ | 538 | | 2·42 | 712 |
| | | | | | 12 | | |
| 0 | 273 | 74529 | 135·5 | 550 | | 2·27 | 713 |
| | | | | | 2 | | |
| 21 | 294 | 86436 | 156·5 | 552 | | 2·13 | 705 |
| | | | | | 15 | | |
| 60 | 333 | 110889 | 195·5 | 567 | | 1·88 | 703 |
| | | | | | 11 | | |
| 100 | 373 | 139129 | 240·5 | 578 | | 1·65 | 696 |
| | | | | | 36 | | |
| 200 | 473 | 223729 | 364·5 | 614 | | 1·37 | 712 |
| | | | | | 5 | | |
| 240 | 513 | 263169 | 425 | 619 | | 1·29 | 713 |
| **II. Pressure 1000 millims. of Water.** | | | | | | | |
| 20 | 293 | 85849 | 110 | 784·5 | | 2·663 | 1035 |
| | | | | | 12·0 | | |
| 40 | 313 | 97969 | 123 | 796·5 | | 2·545 | 1036 |
| | | | | | 10·0 | | |
| 60 | 333 | 110889 | 137·5 | 806·5 | | 2·422 | 1034 |
| | | | | | 7·9 | | |
| 80 | 353 | 124609 | 153 | 814·4 | | 2·307 | 1031 |
| | | | | | 8·8 | | |
| 100 | 373 | 139129 | 169 | 823·2 | | 2·207 | 1031 |
| | | | | | 11·6 | | |
| 120 | 393 | 154449 | 185 | 834·8 | | 2·124 | 1035 |
| | | | | | 7·9 | | |
| 140 | 413 | 170569 | 202·4 | 842·7 | | 2·040 | 1034 |
| | | | | | 3·8 | | |
| 160 | 433 | 187489 | 221·5 | 846·5 | | 1·955 | 1030 |
| | | | | | 9·4 | | |
| 180 | 453 | 205209 | 239·75 | 855·9 | | 1·889 | 1034 |
| | | | | | 9·6 | | |
| 200 | 473 | 223729 | 258·5 | 865·5 | | 1·833 | 1035 |
| | | | | | | | 1033·5 |

These Tables show—

(1) That the time varies approximately as the square of the absolute temperature. This appears from the approximate equality of the numbers in column 5.

(2) That the variation of the time deviates from the law of squares by a term approximately proportional to the temperature. This is shown by the average equality of the differences in column 6.

(3) That the formula connecting the time and the temperature is very nearly

$$t = d\mathrm{T}(\mathrm{T}+\beta),$$

where $\beta$ seems to depend on the pressure $p_1 - p_2$.

From the above series of experiments, however, the exact connexion between $\beta$ and $p_1 - p_2$ is not obvious; possibly it may be of the form

$$t = \alpha\mathrm{T}\left(\mathrm{T} + \frac{\beta}{(p_1+p_2)^3}\right).$$

The next series of experiments had in view the more exact determination of the relation between $p_1 - p_2$ and $t$.

To obtain these results, it was necessary to determine the effect of capillarity and bubble-tension on the exit of the air through the final orifice.

It was found from experiment that these forces were very nearly equivalent to a pressure of 22 millims. The depth of the exit-orifice under the surface of the water over which the air was collected had, of course, to be allowed for.

The experiments gave the following results:—

Temperature 20° C.

| | $p_1-p_2$. | $t$. | $(p_1-p_2)\times t$. | $\Delta(p_1-p_2)t$. |
|---|---|---|---|---|
| | millim. | | | |
| I. | +15 | 3945″ | 59175 | |
| | 76 | 719 | 54644 | −4531 |
| | 157 | 344·5 | 54086 | − 558 |
| | 303 | 180·5 | 54691 | + 605 |
| | 513 | 105 | 53865 | − 826 |
| | 656 | 81 | 53136 | − 729 |
| | 852 | 61·75 | 52611 | − 518 |
| | 1205 | 43·6 | 52538 | − 73 |
| II. | 200 | 426·5 | 85300 | |
| | 400 | 209·5 | 83800 | −1500 |
| | 600 | 139 . | 83400 | − 400 |
| | 800 | 103·6 | 82933 | − 467 |
| | 1000 | 82 | 82000 | − 933 |
| III. | 200 | 563·6 | 112733 | |
| | 400 | 278 . | 111200 | −1533 |
| | 600 | 184·6 | 110800 | − 400 |
| | 800 | 137·3 | 109866 | − 934 |
| | 1000 | 108 | 108000 | −1866 |
| | 1200 | 89 | 106800 | −1200 |

The approach to equality in the amounts in column 3 shows that the time varies nearly in the inverse ratio of the difference of pressures. The gradual diminution of the number in that column visible, on the whole, shows that this law is slightly deviated from, and that the time diminishes somewhat more rapidly than the reciprocal of the pressure—a conclusion which is at variance with the results arrived at by Bunsen for the passage of gases through porous plugs.